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THE DIRECT MANUFACTURE OF IRON FROM ORE, PUDDLING, HEATING FURNACE AND FORGE CINDERS, SHOWING THE ECONOMICAL ADAPTATION OF THE PROCESS FOR COMMERCIAL IRON FOR OPEN HEARTH AND FOR CRUCIBLE STEEL.

By CHARLES M. DU PUY, C.E.

Read before the Franklin Institute, June 17, 1881.

I have the honor to read a third paper before this venerable and respected Institute upon the manufacture of iron.

It may be remembered that in the fall of 1878 I explained a method of producing wrought iron direct from ore by reducing it with carbonaceous matter and fluxes.

The mixture filled into thin sheet iron cases, and charged into reverberatory furnaces, is subjected to a gradually increasing heat for three or four hours, when it becomes changed to metal interspersed with slag, but still surrounded with the iron cases, more or less intact.

One or more of these metallic lumps, pressed into the shape of a ball, is then shingled and rolled to a bar at the same heat.

About 50 tons of iron was made by this method from ore, then piled, reheated, rolled, cut up, and melted in crucibles to steel. After a very careful test of this steel for various purposes, the stock was pronounced

by skillful manufacturers equal to the best American or Swedish iron for the finest grades of steel.

A year later I read a second paper reporting the result of further investigation. During this interval a variety of ores had been worked. They were deoxidized not only with charcoal, as the first had been, but also *with bituminous and anthracite coal.* Some of these samples reduced with mineral coal, after reheating and rolling to bars, were converted to crucible cast steel and forged to tools. The result proved that whether deoxidation was produced with charcoal, with bituminous, or with the wasted anthracite coal slack dust of our mines, a high quality of steel was always produced, the tools from which have withstood the severe test of turning chilled rolls and planing hard cast iron.

With this brief reference to former papers, which have been printed in your JOURNAL, I pass on to detail further progress. Since that period various practical operations have been conducted for weeks together at different rolling mills, in charging ore mixtures in sheet iron cases upon the hearths of reverberatory furnaces.

After having fully demonstrated that a simple and cheap combination of fluxes with ore and carbon will always produce a uniform and satisfactory quality of metal for steel purposes, my attention has been closely directed to the commercial side of the operation. My aim has been to prove that this system can take its place in economic competition with the other older methods now in use.

In the course of these experiments I discovered that sheet iron cases may be dispensed with, without prejudice to the yield and quality of the metal. This discovery is important, as it sums up a large saving in the cost of production.

I early found that complete reduction in reverberatory furnaces could not be accomplished through a thickness of three or four inches of pulverized ore mixtures in as many hours, and yet, unless the process is hastened to a much shorter time, experience has proved that the waste of fuel, metal and labor will determine its commercial uselessness.

It was to overcome this difficulty that *annular* sheet iron cases were adopted, with a thickness of not more than five inches between the outside and inside walls of the metal mixture. By this arrangement the heat was not required to travel over 2½ inches in order to penetrate throughout the mass.

Although sheet iron cases are now dispensed with, the mixture is still moulded by *preference* into cylindrical annular shapes, as the pipe form is considered best for their rapid and cheap manufacture by machinery, as drain pipe are usually made.

It is quite important to have the mixture moulded to shapes, so that by standing them on end throughout the furnace hearth a sufficient *paying* quantity can be operated upon at one heat, while at the same time there is presented large surface areas for heat penetration.

Pulverized ore mixtures, leveled over the hearths of reverberatory furnaces, have often been reduced and *balled*, and if not over two or three inches of thickness, iron is usually made, but as the mass is heated only from the upper surface, the bottom being cold, the prolonged high heat required to penetrate this non-conducting mixture causes excessive waste both of material and labor, together with a small yield of metal.

In previous papers I attempted to explain that the greatest impediment to commercial success of direct processes has been, if I may be allowed so to express it, the failure to realize *the low heat conductivity* of ore mixtures. The reducing heat has been expected to penetrate more rapidly than the nature of the material will conduct it.

The thin annular cylindrical shape overcomes this difficulty. They are made about 15 to 18 inches high, 8 inches diameter and $2\frac{3}{4}$ inches thickness, and placed throughout the hearth so as almost to touch each other. Thus arranged, the furnace will produce as much wrought iron from ore at a heat of no longer duration than is required to produce the same quantity from pig iron.

It is not necessary to be confined to the annular cylindrical shape, although, perhaps, it may ultimately be found the best. Any form that may be easily handled—that will have sufficient base to afford a firm support in the furnace—and that will be thin enough to present large surfaces for heat penetration, will answer the purpose. They may be moulded to form on end, the shape of the letter C, a cylinder almost closed. They may be S shape or D shape on end. Many forms may suggest themselves, as easily moulded by machinery, which will be thin, and yet present large surface areas for penetration.

Formerly, in making my experiments I used reverberatory furnaces with *sand bottoms*, similar to those for heating iron piles. In one respect these bottoms are favorable, for the silica becomes quite hot, and gives off its heat to the metal mixture placed upon it, but further

experience has proved that the alkali mingled with the ore to separate impurities, after a very few operations will partially dissolve the silica bottom. The metal will then mingle with it, and much of it waste away as a silicate of iron. For this reason silica bottoms have been discarded for the ordinary puddling furnace *cinder bottoms*, and these are found in every way satisfactory. They keep the metal clean, and are themselves uninjured by use.

In preparing the mixture several points are necessary to be observed, and, although they are subsidiary to the separation of impurities, are still very important, if not essential, to the complete success of the system.

The mixture must be so compounded as, when moulded, to withstand the shock of rough handling without breaking in transportation to the furnace.

It must also be of such consistency as to preserve its original moulded shape, while in the furnace, until thoroughly penetrated by heat. If it settles down into a mass of uniform thickness over the hearth, before it is penetrated by a reducing heat, the cost and yield will render the process unprofitable.

Both of these conditions are attained by a judicious mixture of lime and clay with pulverized ore and carbon, when a preponderance of silica is combined with the ore. Magnesian lime is preferred. With aluminous ores the clay may be largely dispensed with. These substances must be varied in proportion with the analysis of the ore, but they may always be so combined as to make a firm compact mass at low cost. Salt and manganese have been used sometimes in addition to lime and clay to aid in separating phosphorus and other impurities, but lime and clay are the main dependence.

There is one exceedingly important office of this cheap alkali mixture to which is due almost entirely the commercial economy of the process. It must form a "*non-flowing slag.*" Not simply a flux to dissolve impurities, but a combination which fills a range of usefulness very much wider than that of any ordinary flux.

As the moulded masses are spread over the hearth with spaces between them, they present not only larger surface areas for *deoxidation*, but also present shapes that would be very favorable for *re-oxidation* by the furnace gases, unless means can be provided to prevent it. While it is essential to drive the heat high from the beginning, in order to bring about quick and economical reduction to metal, it is

equally as essential that the material shall at the same time be covered and protected from the waste of high heat, which is always very oxidizing. This is one of the requirements of this "*non-flowing*" slag. By entering the pores or spaces or cells of the ore, which have been vacated by the oxygen as it passes out and forms with carbon, carbonic oxide gas, each little atom or filament of metal is immediately sealed and varnished by this glassy coating, so that the delicate particles of new-made iron are in this *manner effectually saved from re-oxidation and destruction.*

An *ordinary* flux would flow from the metal on to the bottom of the furnace, carrying, it is true, a portion of the impurities with it, but leaving the new spongy iron to melt into a protoxide, like snow melts before a south wind. Hence the importance of having this slag non-flowing, as well as fluxing, which is so very easily accomplished.

There is still another result accomplished by the proper mixture of these cheap alkali material with ore and carbon. *Being highly basic, it dephosphorizes and desulphurizes as well as desiliconizes.* This same combination, which may be depended upon to form a firm mass both out of and in the furnace, and that protects the ore from oxidizing influences by furnace gases, may be also effectually relied upon to break the affinity of iron both for phosphorus and sulphur as well as for titanic acid. A large class of rich ores may be thus utilized which are now useless, because they contain so large a percentage of these impurities. These deleterious substances are separated either by volatilization or pass off with the slag as it is expelled by the squeezer and rolls.

Still I am not yet through with extolling the virtues of this "*non-flowing slag.*" Important as its influence is for the purposes here before stated, it also serves to secure a great economy of fuel. Metallurgists and chemists agree that ore should be brought to wrought iron bars with the consumption of half a ton of coal to the ton of iron. Notwithstanding these nice theoretical estimates, the best part of three tons of coal is required to a ton of bars. This is one of those marked cases where theory and practice widely differ, in spite of every effort to correct it. Some very powerful, but perhaps not very well understood, cause must lie at the bottom of this extraordinary waste in converting ores to metal. Allow me to venture the query whether it may not be produced by *nitrogen.* The chemist will create intense

combustion in a jar of pure oxygen, but let him mingle with it a large volume of nitrogen, and combustion will be arrested.

Until lately nitrogen has been considered rather a neutral substance in iron making, being neither favorable nor injurious to the metal. Now with every 1000 pounds of air thrown into the furnace, only 230 pounds is oxygen, while of the remainder more than *700 pounds is nitrogen.* The oxygen forms not one-fourth of the total blast, and yet it is only upon the oxygen we place dependence to secure combustion. A gas which forms nearly three-fourths of the entire volume contained in the furnace, if it is not combustible itself, most certainly must retard the ignition of the other combustible gases.

May it not be possible that these imprisoned gases in the molecules of metal are the prime cause of deteriorating its quality, so that puddled pig iron will so rarely produce high grades of crucible steel? Dr. Müller, of Brandenburg, has proved by a simple and ingenious method that hydrogen and nitrogen are actually contained in very considerable quantities in iron. He practically determined that in some cases these gases formed a volume of about fifty per cent. of that of the drilled hole from which the test was made, and these experiments have since been verified by others.

In the process under consideration *there are no* gases forced by the *pressure of the blast through the metal as in the blast furnace.* As the carbonic oxide is generated by the heated mixture, by its own pressure it finds its way to the surface of the moulds. The door is open for the exit of gases, but is closed and sealed against the inroad of injurious volatile furnace impurities. The blast pressure is not sufficient to counterbalance the outward pressure of carbonic oxide, and hence gases are not introduced into the recesses of the metal to its injury, as pig iron is contaminated.

Whether the foregoing is or is not a correct solution of the problem, it is very certain that iron produced by this system is invariably of a quality that may be relied upon for the finest grades of fine crucible or open-hearth steel.

In all previous direct processes it has been considered essential to keep the furnace in a red smoky atmosphere of carbonic oxide, as the best condition for rapid reduction. This is a condition, it may be remarked, where about one-half of the fuel passes unconsumed out of the stack, and the other half is not allowed to generate more than half the heat it is capable of, if sufficiently supplied with air.

In common with others, I formerly deemed this most wasteful combustion of the fuel to carbonic oxide an essential means for the best reduction. Although I had discovered that commercial economy necessitated the application of a *high furnace heat at once*, in order to penetrate to the interior and rapidly reduce to metal, yet I was unable to apply this high heat because of its waste by a re-oxidation of the iron. After I found that a non-flowing slag could be made to remain and protect the metal, I was able to push the heat high on introducing the charge; and thus, by more economically burning the fuel to carbonic acid, bring the ore to metal very rapidly in less than two hours. I am aware that the treatment of an ordinary puddle ball to such prolonged exposure to furnace blast would oxidize and waste it largely to cinder, but it must be remembered that this combination is not that of an ordinary puddle ball. It is iron ore mingled with carbon to deoxidize it, and protected at every point by a glazing slag which prevents re-oxidation.

Saturated, as the mixture becomes, with a high heat, as fast as it can be conducted through it, the gases are observed to begin to work immediately. Every square inch of surface of these moulded masses is covered with a flame of carbonic oxide which is quickly transformed to carbonic acid upon meeting the heated furnace gases. Thus combustion is intensified by the heat evolved from the moulds themselves, throughout the entire furnace hearth. The heat given off from one mould impinges upon the mould adjoining, while the furnace walls and roof receive and reflect back the radiated heat. Every little particle of ore gives off *pure oxygen*—not oxygen diluted with three-quarters nitrogen to retard perfect combustion, as in the blast furnace—but as pure oxygen as it is possible for the chemist to generate in his laboratory. This oxygen immediately finds an atom of carbon close to it with which it assimilates and forms carbonic oxide. Thus existing gaseous impurities are removed, while other gases injurious to the iron are not *insidiously introduced with a strong blast pressure* as they are in pig iron. The practical result follows, as has been before stated, the iron is better for fine steel than puddled pig iron.

It is no longer necessary to waste fuel, ore and labor by maintaining in the furnace an atmosphere of carbonic oxide, which so needlessly prolongs the operation. The moulded mixture, upon the application of high heat, is itself a flame at all points, like dry wood

would be under similar circumstances. They produce, of *themselves*, a high heat, and thus economize fuel from the fire-grates.

Indeed, so effectual is the heat generated at the surface of the moulded mixture that I am accustomed to lessen the supply of fuel at the stoking-hole as the gases begin actively to work, and then the blast is reduced while the damper is somewhat lowered. Under this treatment, the ore mixture continues to do its work until the whole is reduced to metal. It asks for no puddling, no exhausting physical labor, but simply to be let alone to do its own work in its own way.

I may have been tedious in detailing the value I have discovered there is in a non-flowing slag, and also in so minutely describing the working of the process, but I have purposely elaborated this part of the subject, at the risk of wearying you, because I believe *the true secret* of successful commercial economy in working the direct method lies in this direction.

There is nothing new in fluxes, nor in moulding the mixture into shapes for reduction. The few alkali or acid materials cheap enough to be used for fluxes have all long ago been known, and one inventor after another has used them in varied proportions in moulded shapes, and still failed to demonstrate commercial economy in competition with other methods of working. Like my own earlier experiences, the successive steps to bring ore to metal direct have not been well understood. A non-flowing slag to protect from re-oxidation, and a quick, high heat, seems to be the pivot upon which commercial economy turns. By coupling this with that other quite as important discovery —namely, placing the several masses of metal mixture sufficiently apart so that the heat generated from one may impinge upon another— the general outline for practical working seems to be complete. A wide range of experimental tests with ore mixtures, in cases and without them, and in various furnaces, have gradually pointed out the successive steps by which to make the method simple and easy. Now heat after heat may be withdrawn from the furnace in less than two hours with the regularity of puddling, but with very much lessened cost.

Having succeeded in satisfactorily working ore-mixtures, my attention has latterly been directed to the utilization of puddle, forge and heating furnace cinder as well as roll scale and hammer slag.

Puddle cinder is a rich silicated protoxide of iron, containing from 50 to 58 per cent. metallic iron, 16 to 18 per cent. of silica, and from

1 to 2 or 3 per cent. of phosphorus, beside some other impurities. An *average* of this cinder, both here and in Europe, would probably prove it to contain not less than 52 per cent. metallic iron. This class of cinder, in this country, is quite high in phosphorus, because the pig metal is boiled purposely very hot in order to throw the phosphorus as much as possible into the cinder for the better purification of the iron.

Heating furnace cinder is not generally as rich as puddle cinder, still it averages a large percentage of iron, probably not less than 45 per cent., while some of it, taken from furnace bottoms, is even richer than puddle cinder.

Forge cinder, the refuse of charcoal scrap sinking fires, and from Catalan forges, is a richer iron oxide than puddle cinder, and besides contains a very considerable quantity of reduced iron blended with it, which, by the usual method of working charcoal forges, it is impossible to separate.

Roll scale is a pure oxide of iron, containing, as gathered up in the mills, about 70 per cent. of metallic iron. Although apparently little scale is made from iron in hammering and rolling it, yet in large mills the annual quantity, when summed up, would be quite surprising. Where puddling is carried on, scale is often used by the puddlers at certain stages of the boil, but even in many of these mills the scale is swept up and thrown with the cinder.

Hammer and roll slag are both rich in iron, averaging perhaps nearly as much metallic iron as heating furnace cinder, say 40 to 45 per cent.

It is quite worthy of note that all these refuse slags, so very rich in metallic iron, and produced in so very large quantities, are often wholly wasted, or, if utilized, only command a fraction of the market value which the same number of units of iron would realize if contained in ore. Most blast furnace owners would rather pay several dollars per ton for ores containing as low as 30 per cent. of metallic iron than to charge their furnaces with cinder, to be had for the hauling, that analyzed 54 per cent. of metallic iron. Owing to the alloy with phosphorus, and the very refractory nature of these cinders, furnace managers will at best only use them sparingly. Many who are proprietors of both rolling mills and furnaces prefer to waste their rolling mill cinder on the dump rather than use it in their blast furnaces, for fear of contaminating a superior grade of pig iron they aim to produce. Even in furnaces not very particular as to quality of

iron, the use of cinder is usually limited to from 10 to 20 per cent. of the charge, the latter being rather an unusual quantity for ordinary brands of pig.

Not less than 40 to 50 tons of puddle cinder daily made by the Philadelphia rolling mills is given away for the hauling and barely pays for the labor of transportation to cars a short distance from the mills, to be delivered to neighboring blast furnaces. By this process this wasted cinder could be daily converted into 12 to 15 tons of good wrought iron at low cost.

In former years cinder pig iron was a marketable commodity, very much in demand for working into cheap rails, etc., but now ingot iron and low steel is so rapidly pushing it away, and displacing puddled iron in rolling mills, substituting superior for inferior stock, that cinder pig iron is no longer in request. With an increasing demand for better qualities, the less cinder can be used in blast furnaces, and the more it will accumulate. In Staffordshire and many other parts of England, at the present time, cinder has fallen so much in demand for furnaces that many rolling mills are seriously inconvenienced by the large accumulation of cinder around them. Indeed in some parts of Europe, and even in this country, cinder has been used to fill up vacant lots and low places, and sometimes even forms the embankment of railroads. In Sweden rich forge cinders, unused, have been accumulating in enormous piles for 50 years.

I have now succeeded in working these cinders, so rich in iron, and rolling them to bars at one heat. I prepare this material with carbon and substantially the same cheap fluxes as used for ores. It is safe to estimate, under all contingencies, that one ton of muck bar can be made on a regular working scale from three tons of puddle cinder, deoxidized with bituminous or anthracite carbon in heats of from 500 to 1000 lbs., according to the size of the furnace.

During a series of experiments at Round Oaks, Staffordshire, England, in the past winter, I produced as high as 43 per cent. by weight of muck bars in $2\frac{1}{4}$ hours from $\frac{3}{4}$ cinder and $\frac{1}{4}$ scale, deoxidized with bituminous carbon. In this instance the muck bar, with one-half scrap, has lately been cut up, sunk in charcoal, forged, reheated, and rolled to $1\frac{1}{8}$ inch wire rods, showing no greater waste than usually attends that mode of working iron. These wire rods were then reheated and rolled down to No. 6 wire. The wire was salt coated and in four passes was drawn to No. $12\frac{1}{2}$, after which it was annealed

and meal coated, and in two more passes it was drawn to No. 16 wire. No better illustration than this can be presented to show the character of iron made from cinder by this method. The cinder from which this iron was made contained more than 2 per cent. of phosphorus, whereas an analysis of the iron showed it had been reduced to $\frac{38}{100}$ per cent.

Within three months, at the Phœnix Iron Company's works, 27.426 lbs. of puddle cinder, 7350 lbs. of old bed Champlain Ore, and 2400 lbs. of iron scale was mingled with bituminous coal and slagging material. The combinations were varied, and it was then moulded into shapes for testing the system. The charges were placed in an ordinary double puddling furnace. Some of them were of cinder alone, some cinder and ore, and others cinder and scale. In this way a variety of tests were made, each with a sufficient quantity to run the furnace several heats upon one mixture. Part of the time the furnace was run day and night continuously, making then 6 heats in 24 hours, averaging 507 lbs. of muck bar at a heat. Since then the heats have been much shortened.

The mixtures were all moulded 15 inches high into the annular cylindrical shape, being $8\frac{1}{2}$ inches outside diameter, and cored from top to bottom 3 inches diameter, and for more uniform heat circulation they were cross cored through and through at the base. With cinder mixture they contained each 48 lbs. of cinder, and of ore alone 52 lbs., while the weight varied between these points in proportion to the variation of the ore and cinder mixed together.

In every one of these tests the iron oxide was reduced to metal, balled, squeezed in a rotary squeezer and rolled to muck bar at the same heat, presenting in lengths and appearance the ordinary muck bar from pig iron. The average period of the heats was 3 hours, while the yield of the whole in weighed muck bar was about 32 per cent. of the weight of the oxide.

At the previous test, before referred to, the heat was $2\frac{1}{4}$ hours and the yield larger. This is accounted for mainly by the lesser thickness of the moulded shapes, which admitted more rapid heat penetration. The cost of moulding pipes of the less thickness by machinery is comparatively trifling in comparison with the quickened production and reduced cost of the metal.

A greater advantage will be gained in doubling the hearth capacity to twice that of the ordinary double puddling furnace. Then 1000

lbs. of iron, with no more labor, may be as easily produced at a heat as 500 lbs. has already been done with the present size.

Such increased furnace proportions would not be as economical for puddling pig iron, but as by this system the carbonic oxide generated by the metal mixture itself is largely depended upon as a heating power, and it is distributed over the entire hearth surface, the conditions are quite different from puddling pig iron. In this case the pure oxygen of the metal mixture from all parts of the hearth readily finds for itself (without furnace labor upon it) atoms of carbon in close contact with which to assimilate and generate heat.

In puddling pig iron the 2 or 3 per cent. of carbon it contains is prevented from escaping by the melted metal. Its exit is sealed, or at least very much retarded, by the *weight of the melted metal*, and it is only after excessively laborious "rabbling" or stirring that the way is opened for its escape. Thus allowed to volatilize, it combines with oxygen into carbonic oxide, but as three-fourths of the furnace gas is nitrogen, for which carbon has no affinity, it becomes necessary to pass large volumes of air very rapidly through the furnace, in order to have sufficient oxygen present with which the carbon may combine. With the swift movement of these air currents, sweeping like a whirlwind through the furnace, much of the carbon from the pig is passed off undecomposed, and along with it vast volumes from the fuel, rendering the top of the stack a constant monitor of wasteful combustion.

The following analyses have been made by the direction of David Reeves, Esq., President of the Phœnix Iron Company, by whose kind permission I use them.

An analysis of the 27,425 lbs. of puddle cinder referred to as recently worked proved its composition to be

Silica,	17·710
Iron,	54·290
Phosphorus,	1·960
Sulphur,	·280

The ore used was magnetic known as "old bed Lake Champlain," such as is usually used for fix, and highly charged with phosphorus.

Fluxes and bituminous coal were mingled in different proportions with a varied combination of cinder, scale and ore, and consecutively numbered.

No. 1 was composed of 6000 lbs. of cinder alone. An analysis of the 1690 lbs. of muck bar produced from it showed it to contain

Silica,	·48
Phosphorus,	·40

No. 2 was a mixture of two-thirds cinder and one-third "old bed Champlain" ore, making 6000 lbs. in all. The 1644 lbs. of muck bar it produced analyzed

Silica,	·45
Phosphorus,	·37

No. 3 was of 6000 lbs. of cinder alone, but fluxed differently from No. 1, and the analysis of its 1853 lbs. of muck bar was

Silica,	·35
Phosphorus,	·38

No. 4 was composed of 1200 lbs. "old bed Champlain" ore and 4800 lbs. of cinder. The 1842 lbs. of muck bar from it analyzed

Silica,	·55
Phosphorus,	·55

No. 5 was a mixture of 2400 lbs. iron scale and 4800 lbs. cinder. The 2183 lbs. of puddle bar it produced analyzed

Silica,	·29
Phosphorus,	·36

No. 6 was exclusively of 4673 lbs. of "old bed" ore, whereas the analysis of 1437 lbs. of puddle bar from it showed

Silica,	·62
Phosphorus,	·16

No. 7, with a different mixture of fluxes from any of the others, was composed entirely of 1887 lbs. of cinder. The 537 lbs. of muck bar analyzed

Silica,	·26
Phosphorus,	·38

Some important conclusions may be deduced from these experiments. For instance, in the mixture No. 3, the 6000 lbs. of puddle cinder contained, by analysis, 17·7 of silica, or 1062 lbs. of the entire weight, whereas the analysis of its muck bar showed it contained only $\cdot\frac{35}{100}$ of one per cent., or $6\frac{1}{2}$ lbs. This is an elimination of 1056 lbs. of silica from the 1853 lbs. of muck bar produced from this mixture.

Again, the analysis of this No. 3 puddle cinder showed it contained 1·96 per cent. or 117 lbs. of phosphorus, while the analysis of the muck bar contained only $\cdot\frac{38}{100}$ per cent. or 7 lbs. Here is an elimination of $110\frac{6}{10}$ lbs. of phosphorus out of $117\cdot\frac{6}{10}$ lbs. which the 1853

lbs. of muck bar, made from the 6000 lbs. of cinder, originally contained.

By referring to my first paper, it will be observed that an analysis of a *finished* bar, made by this process from Republic ore, showed an elimination of $\frac{4}{5}$ of the phosphorus, but the foregoing shows even a better result from the muck bar from *puddle cinder* carrying $1 \cdot \frac{96}{100}$ per cent. of phosphorus, while *Republic ore* only contains $\cdot \frac{053}{1000}$ per cent. I deduce from this that the proportion of phosphorus eliminated, whether the quantity be large or small in the ore or cinder to be worked, will always be such *as to give value to a highly phosphoritic metal mixture reduced to iron by this process.*

This system is also applicable, not only for iron oxides, but for all other metallic oxides requiring reduction.

An examination of the entire seven analyses shows results substantially alike, and proves how thoroughly impurities are separated in a few hours by this method.

Here is another important deduction, proving that even low-grade ores may be profitably worked upon this system. The mixture No. 4 was composed of 4800 lbs. of puddle cinder and 1200 lbs. of "old bed" ore. To this 6000 lbs. of metal mixture was added $12\frac{1}{2}$ per cent. of earthy matter, so that the entire quantity of earthy matter contained amounted to 1617 lbs., while the 1842 lbs. of muck bar produced from it contained only ·55 per cent. or $10\frac{13}{100}$ lbs. of earthy residuum. An elimination of 1607 lbs. out of 1617 lbs. originally contained in the metal mixture.

This would show that ore or puddle cinder, containing 30 per cent. of earthy matter in all, can still have it eliminated down to $\frac{55}{100}$ of one per cent., and I believe by a slight increase of lime it may be *entirely separated.*

This fact is worthy of consideration in connection with the low-grade ores which so generally abound. Throughout the coal measures this class of ores are found, closely interstratified with the fuel, and by reason of this proximity produce iron at low cost. Yet because of the preponderance of sulphur and phosphorus, they are not satisfactorily worked alone in the blast furnace.

Turning to the recent Geological Survey of Pennsylvania, vol. "MM," page 179, I find an analysis of Westmoreland county ore containing 41 per cent. metallic iron and 50 per cent. volatile impurities and less than 10 per cent. of insoluble residue. This is a type of a large

variety of ores in Cambria, Huntingdon and, in fact, throughout the entire bituminous coal fields. Many of these ores contain more than ten per cent. of earthy matter, but rarely exceed 30 per cent., which has been referred to as so satisfactorily worked in the mixture designated as No. 4, where the earthy residuum was reduced to $\cdot\frac{55}{100}$ of one per cent.

The facility of eliminating phosphorus so largely from puddle cinder containing nearly 2 per cent., as has been proved by the foregoing analyses, should be quite conclusive that the phosphorus can be readily removed from these ores of the coal formation when muck bar would then be produced from them at one heat not only at the lowest possible cost but of a very superior quality.

I have dwelt upon this branch of the subject because of its economic importance. If the gaseous and earthy impurities can be so easily removed from low-grade ores, as the result of the analysis of the No. 4 mixture would seem to prove, then the field of usefulness of this system will be much enlarged.

Within the last two weeks, still more satisfactory results have been obtained from working the charcoal-forge cinders at the forge of the Washburn & Moen Manufacturing Company, at Quinsigamond, Massachusetts. From these cinders, 1794 lbs. of blooms were produced from a small sand bottom reverberatory furnace, there being none there with cinder bottom.

The average yield, from the entire quantity of cinder used, was 38 per cent. of its weight in forged blooms. *It was not unusual to shingle blooms of 150 to 180 lbs. in one hour from the time the mixture was charged in the furnace, while the average length of the heats did not exceed $1\frac{1}{2}$ hours.* Four heats were made between ten o'clock and five, being an average of $1\frac{3}{4}$ hours, including the usual delays in repairing a sand bottom. Deoxidation was accomplished by using the fine breeze or dust charcoal, which is now wasted at charcoal forges. In this instance the tubes were 15 in. high, 8 in. diameter and $2\frac{3}{4}$ in. thickness.

The blooms thus produced, while still hot, were for a few minutes reheated and then rolled to muck bars. These bars were cut up and, with an equal quantity of scrap, sunk in the charcoal-forge fire, then forged, reheated and rolled down to $1\frac{1}{8}$ inch wire rods, with no more than the usual waste in sinking iron in charcoal fires.

The rods so produced have all been rolled down to No. 6 wire, and part of it has been drawn to No. 12 size, with the intention to draw it

still finer if the stock will permit. Both the analyses, tensile strength and various further tests of this forge cinder iron is expected to be fully obtained within the next few weeks by this company.

The few experiments herein described have been the first attempt to make wire from cinders to my knowledge, and it is possible that the practical working of it may prove that the metal mixture was not sufficiently *basic* to eliminate enough silica and phosphorus to produce a very fine wire. Should such be the case, the difficulty can easily be removed in the future by slightly increasing the proportion of lime.

There are several elements of economy by this method that cannot be attained by producing pig iron from ore and then puddling it. To produce pig, it is essential to roast refractory and very impure ores and cinders before charging them in the blast furnace. This is required in order to expel volatile impurities as far as possible, as well as to render them more porous for the mingling of the gases.

Roasting is unnecessary for this direct method. By grinding the mixture together, properly proportioned, there is such close *atomic contact* created between the particles that reduction takes place rapidly without roasting. This is one element of economy.

Usually the mixture has been ground as fine as coarse Indian meal, but in the last experiment with forge cinder at Quinsigamond, just referred to, one of the mixtures was composed of one-half the cinder crushed to the size of small beans and the other half as fine as meal. The yield in metal and length of heat did not vary from that of the other forge cinder mixtures there, which were all ground fine, proving that very fine pulverization is unnecessary for forge cinders.

Another element of economy is in the item of fuel. Blast furnaces using bituminous coal require it to be coked, the better to carry the burden and secure greater purity of the gases from it. This operation of coking wastes a large portion of the combustible gases which should be used in reduction. Coking is unnecessary by this new system. Its cost and waste is saved. Bituminous coal, both for deoxidizing and fuel, is used without coking, and repeated analyses prove the iron is freed not only from its own impurities, but that it is uncontaminated by the impurities of the coal.

Still another economy may be counted upon. It is necessary to run the liquid pig iron from the blast furnace into moulds, which are freshly prepared, at considerable expense, for every cast. By this direct system, the metal mixture, *automatically ground, mixed and*

moulded, is charged at once into the reverberatory furnace as has been explained. The entire cost for labor up to this point is less than blast furnace labor.

Pig metal must be broken before charging. It comes to the puddling furnace cold, must be reheated and rabbled with exhausting physical labor, and at an intense heat, in order to separate the intimately combined carbon and silica, before it is changed to wrought iron ready for the squeezer and rolls.

By this system the metal mixture is reduced to a wrought or malleable condition at one operation from the ore. In $1\frac{1}{2}$ to $2\frac{1}{2}$ hours, according to the thickness of the moulded shapes, it is ready for the squeezer and rolls, and is brought to muck bar. It is then not only of superior quality to puddled iron, but is one step in advance of it in the process of manufacture; a step which requires from $12 to $15 per ton outlay on the pig iron to bring it to a malleable condition.

If the balled iron has been made from material nearly free from phosphorus, without squeezing, in its heated state, it may be transferred to and melted in the open hearth bath for steel, when it will separate from its earthy impurities by precipitation. If, on the contrary, the stock originally contained an objectionable percentage of phosphorus, the slag into which the phosphorus has been incorporated must first be squeezed out to guard against a second combination with the metal. Then the bloom, in its then highly heated state, may be at once melted in the bath, with the certainty of being freed from phosphorus. In either case a *superior, cheap and uniform quality* of metal is assured, adaptable, at pleasure, to any purpose required, from the best ingot iron, low in carbon, to the finest grades of high steel.

Practical tests, on a regular working scale, have thus far proved that the metal by this system will produce the finest grades of crucible, or open-hearth steel. Sunk in charcoal it makes fine grades of tough sheet iron, both of black and planished iron. Tests, not yet fully completed, are sufficiently advanced to prove that it will make a good quality of wire. In addition to this, as it can be proved that its cost is much below the cost of ordinary scrap or puddled iron—that its mode of working is simpler—that the cost for plant for a given quantity of iron or steel is very low—and that impure ores or cinders may be worked with safety, the process may reasonably commend itself to the close investigation of manufacturers.

DISCUSSION

*Of the Papers of C. P. Sandberg on "Rail Specifications and Rail Inspection in Europe," of C. B. Dudley on the "Wearing Capacity of Steel Rails in Relation to their Chemical Composition and Physical Properties," and of A. L. Holley on "Rail Patterns," at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17th, 1881.**

ASHBEL WELCH, Lambertville, N. J.—Dr. Dudley has given the wear of steel rails under four different conditions. He arrives at the conclusion that the softer rails, or those that from their composition ought to be softer, wear better than the harder. But there is another condition which has an important bearing on the subject, and should not be overlooked—the weight on a wheel. With the lighter weights of the past, the softer rails may have worn best; with the heavier weights of the future the harder may wear best. Weights will probably be increased up to the capacity of steel to bear; then, doubtless, the harder steel will wear best.

A leaden rail, with 10 pounds on a wheel, might carry millions of tons, but with 100 pounds on a wheel it would be destroyed by a few thousand tons. So in the days of iron rails, my experience was that the softer rails under light machinery stood better than some of the harder; but under heavy machinery the softer were much the most rapidly destroyed. It is doubtless the same with steel.

The pounding motion of the wheels loosens or spreads the particles of a thin film of steel; the pull lengthwise on the rail detaches or scrapes them off. The softer the metal, the more liable the particles to spread or flow sideways; the more brittle, the more liable the particles to break loose. With light machinery, flowing may be practically nothing, with heavy machinery it may be enough to wear the rail out

*The remarks as here given, as in the previous discussion of Dr. Dudley's papers, have all been written out or revised by the participants in the discussion, and represent, therefore, their mature views. It has been thought that this plan, when it can be carried out without doing any of the speakers injustice in debate, is much to be preferred to a strictly verbatim report. The remarks of Mr. Chanute were sent to the Secretary after the meeting, and although they did not form a part of the actual discussion, there can be no doubt of the desirability of including them in this report.

For Dr. Dudley's paper see JOURNAL for March and April, 1881.

very rapidly. At a certain point doubling the weight might increase the flow tenfold. The harder the metal, without decrease of tenacity, or increase of brittleness, the better we should expect it to wear. All may depend on what is left in, or used to make it hard.

Dr. Dudley's observations give us incidentally the difference in wear per million of tons carried, caused by difference of weight on a wheel. On the south or loaded track the gross tonnage was 8,000,000 per annum, on the north or light track 5,000,000. As about the same number of wheels must have gone one way as the other, the average weight on a wheel must have been 60 per cent. more on the loaded track than on the light. The wear per million of tons gross load, as found by summing up the wear on each rail, averages 31 per cent. more on the loaded than on the light track. So, in this case, the wear per ton of gross load increased as the 0·6 power of the weight on a wheel. With iron rails in former years it increased much faster than this rate. As machinery becomes heavier it will doubtless increase faster with steel.

As the same engines and tenders with the same weights on a wheel passed over each track, and as the speeds were probably greatest on the light track, the difference in wear due to difference in weight on the freight-car wheels was probably greater than above estimated. The former weights on a freight-car wheel was about $\left(\frac{20000+20000}{8}\right)$ $=5000$ pounds. The weights now coming into use are about $\left(\frac{24000+40000}{8}\right)$ $=8000$ pounds, an increase of 60 per cent. over the weights when the wear reported took place. This may entirely change the relative rates of wear of hard and soft steel.

Several interesting inferences seem to be deducible from Dr. Dudley's observations, but I confine myself to the single point I have made. Ten years ago I found the wear of steel on the roads between Philadelphia and New York about 50 per cent. more than Dr. Dudley found it on his road. This is accounted for by the better road-bed and smaller proportion of passenger trains at high speed on the Pennsylvania Railroad, by the narrow-gauge cars on wider gauge roads as it then was, and especially by the steel rails between here and New York being then only in the worst places. The softer Sheffield rails, made from Swedish pig, wore better than either of the two harder kinds I got from France.

Dr. Dudley's conclusion seems to be that rails should approach the condition of Bessemer iron. I found that the wear of iron rails from Bethlehem was about 25 per cent. more than that of steel from Sheffield, laid in the same track and under the same circumstances. This, however, does not show the relative durations of the rails, for steel, owing to its elasticity, is only injured on the surface, and will wear till it is reduced to a skeleton; while iron is affected all through by each blow, and will finally go to pieces before it is worn down. The point I make is, that though the softer steel may have worn best under the lighter machinery of the past, it does not follow that it will wear best under the heavier machinery of the future.

The patterns of steel rails now displayed, and the papers of Mr. Sandberg and of Mr. Holley, with the allusion that has been made to the history of the now accepted forms, make it proper to give a short historical notice of the first rails which had certain characteristics common to all these patterns, and of the principles on which they were made.

The early steel rails, copied after the iron, had very heavy bases and stems, and no flat surfaces for fishing. One-fifth to one-eighth of the metal put into them did little or no good, and the fish splice, then coming to be recognized as the best, could not be used to advantage. The useless weight made steel rails so expensive that they came into use very slowly. In 1865 I made a pattern to avoid these faults, guided by the following considerations: The theory then was, and I suppose is yet, that a blow on iron such as that given by a locomotive wheel, is felt all through the metal, and produces a permanent though minute disintegration or change of form, the accumulations of which must in time weaken and *unweld* the base and stem as well as the head of the rail, and so extra metal must be put into the lower part of the rail to compensate for this gradual weakening; but that in steel, owing to its elasticity, such a blow produces no permanent effect on the metal, except at the surface. Therefore, the stem and base of steel may be very much lighter than of iron, not only on account of its greater strength, and freedom from welds, but also from its immunity from deterioration by use. Hence, all the metal possible should be in the head where the wear is; and as little as consistent with safety in the stem and base where there is no wear, and with steel no deterioration. In England, where the supports are far apart, strength and stiffness are primary considerations; in this country, where the supports are

close together, other considerations engross attention. The width of the base should be determined by the endurance of the wood it is to set on, and for this purpose must be much greater than is necessary for strength or to prevent upsetting. Partly from calculation and partly from observing the behavior of iron and steel in circumstances somewhat similar, I made up my mind that three-eights of an inch thickness of stem was ample to bear the weights and the shocks, vertical and lateral, of the machinery then in use, and that an eighth thickness at the edge of the base was sufficient to transmit to the wood all the pressure its fibres would bear.

In accordance with these views, but conceding something for the sake of abundant safety and the facility of manufacture, I made a pattern with 4 inches height, 4 inches base, head $2\frac{3}{8}$ wide $\times 1\frac{1}{4}$ deep, stem 7-16 thick, and edge of base 3-16 thick, weighing 53 pounds to the yard. Assuming that fishing made the best joint, I made (as I had previously done in very slender iron rails) the under side of the head and top of the base plane surfaces, as broad as possible, so as to give a perfect and broad bearing to the edges of the fish-plates, and as near horizontal as possible, so as to lessen the tendency of the fish-plate to work out by the jar.

This pattern was condemned by every engineer to whom it was shown, on the ground that it was too weak, and could not be rolled. I was fully aware of the difficulties of manufacture, but was confident they could be overcome. In one point I yielded too much to the manufacturers—rounding off the corners on the under side of the head.

After long negotiations, an order for 200 tons for trial was accepted by Naylor & Co., in August, 1866, and sent to England to be executed. There everybody that saw it condemned it, and for several months John Brown & Co. refused to roll a rail of such a preposterous shape. As I wished to have an extreme test of the correctness of my ideas, I insisted on the performance of the contract. At last the rails were rolled, tested by Kirkaldy in London, and in the spring of 1867 laid down in some of the hardest places between this city and New York. Several hundred tons of the same pattern were laid the next season. I continued to watch and test them carefully for eight years, and so far as I could find not one ever broke, bent or compressed in the stem, or gave out in any other way.

Thus the principles on which this pattern was made, and its proportions with the machinery then in use, were shown to be correct as

seen from the consumers' point of view. It had as big a head and wore as long, and was as free from accident as previous steel rails 20 per cent. heavier. Some manufacturers in this country condemn the pattern because, without trial, they believed it could not be safely rolled. But John Brown & Co., after trial, believed it could be, and showed their confidence by soliciting an order for 10,000 tons more of the same pattern.

The principles of this pattern, with the proportions slightly modified, were gradually, and are now generally, adopted. Mr. Hinckley, after a year's observation, adopted the pattern with a very slight addition to the metal below the head, and in 1868 relaid one track with it from this city to Baltimore, where the rails may still be seen.

In 1874, Mr. Chanute on the Erie, and Mr. Sayer on the Lehigh Valley, simultaneously, and without concert, adopted the sloping sides of the head, which important feature is now in general use.

In 1870, Sandberg's patterns were published, embodying the same principles with the angles and proportions slightly different. Whether he knew what had been done before I do not know. Doubtless the same considerations on which I acted occurred to many others.

As the machinery is now much heavier than in 1866, and steel not now so good as that made by John Brown & Co. from Swedish pig, and as the price of steel is very much lower, a small saving in weight is of less importance. I have nothing to say against the somewhat heavier proportions now generally used; for example, $\frac{1}{2}$-inch stem instead of $\frac{7}{16}$ and $\frac{1}{4}$-inch thickness at edge of base instead of $\frac{3}{16}$.

In Sandberg's new patterns of 1878 he has, however, adopted almost the identical proportions I used in 1866. His thickness of stem is exactly, and thickness of the edge of the base is very nearly the same. His fishing angle which, in 1870, was 22°, is now 30°; mine was always 28°. It is gratifying to find that so able an engineer, after so wide and so long experience, has within the last three years settled down upon the proportions I adopted fifteen years ago, and which, before they were tested, were so universally condemned, especially in the very dimensions now adopted by him.

I think Sandberg's heads are too convex, and his bases rather narrow. In America bases are always far wider than stiffness and stability required, the practical question being, What width of bearing does the timber require? Where chestnut ties are used the base should be

not less than $4\frac{1}{2}$ inches. There is, therefore, little practical relation between the height and base.

The old plan was to increase every part of a rail much in the same proportion; but each part should be in proportion to what it has to do. The head should be deep in proportion to the amount of traffic and the lowness of the rate of interest on its cost. The body need only be strong enough to carry the head after it is well worn down, and that depends on the weight of the machinery, and in the case of steel has little to do with the volume of traffic, except so far as that affects weight of machinery. As on most railroad systems, the same machinery is used on main lines with heavy traffic, and branches with light, I suggested, in 1874, that each system adopt the same body of rail for both, and make the head deeper on the main lines, shallower on the branches. This plan was adopted on the Pennsylvania Railroad. Sandberg seems to recognize this in his patterns of 1878.

R. W. HUNT, Troy, N. Y.—Again Dr. Dudley presents to our consideration a series of most carefully-conducted experiments, and while I fully appreciate the labor and thought this work has cost him, I must still hesitate to accept his deductions.

This paper differs from the previous one in that it deals exclusively with the wearing qualities of steel rails. Dr. Dudley gives his reason for this change in the following statement: "With the improvement in maintenance of way which has characterized the Pennsylvania Railroad during the last five or six years, the removal of rails from the track from the first two of these causes (*i. e.*, broken and crushed) has, if I am right, quite notably diminished. This certainly is true with regard to broken rails. And if, as time advances, the number of crushed rails shall diminish, both because of the continued improvement in maintenance of way before referred to, and because, owing to improved and better methods at the steel-works, there are fewer crushed rails caused by physical defects in the steel, the question of the wearing capacity of steel rails obviously becomes the all-important one."

Certainly, the condition of the road-bed has much to do with rails breaking, crushing and wearing. If the Pennsylvania and other railroads have done and are doing their part, Dr. Dudley courteously admits that the steel-works have performed part of theirs. But, as far as I am informed, the formulas which they have used have been, and are still, quite wide of the one he continues to recommend. The

rails which are not breaking or crushing to so great an extent as formerly contain higher percentages of both carbon and manganese. I will venture the assertion that Dr. Dudley's road has put in but few rails during the last eighteen months that have not contained fully 0 35 per cent. of carbon and 1 per cent. of manganese. And probably the use of this formula will considerably ante-date the time mentioned. These rails do not break or crush, because they were laid upon a better road-bed, were rolled from sounder ingots, were carefully hot-straightened, and, if I may be permitted a Hibernianism, were cold-straightened while still hot. But how will they wear? For an answer to that we must wait.

I have endeavored to study carefully Dr. Dudley's paper, but have failed to be convinced of the correctness of the conclusions which he draws from the chemical analyses and physical tests. I think I have no prejudice in this matter. The best formula for steel for rails is as earnestly desired by me as by any consumer of such steel. In my judgment, averages in such investigations are exceedingly dangerous, unless made from an immense number of samples taken from metal that has had exactly the same history. By this I mean the different samples ought to have been blown at about the same temperature, cast under the same conditions, heated alike, rolled at the same heat and under the same reductions, hot and cold finished alike, placed upon the same road-bed, and given the same amount and kind of wear. This is almost an impracticable proposition, but the failure to fulfill it can only be compensated by an immense number of other samples.

Some of us told Dr. Dudley before that his twenty-five samples were too few upon which to build up a theory, and it seems a little ungenerous to make the same charge against his present sixty-four; but I must do it. These sixty-four tests are not taken from rails which have been subjected to the same conditions; on the contrary, "sixteen of these rails were taken from level tangents and sixteen from level curves, eight from the high side and eight from the low side of the curves. Again, sixteen rails were taken from grade tangents and sixteen from grade curves, eight from the high side and eight from the low side of these curves." Asking for so great a number of tests means a tremendous amount of work and patience, but Dr. Dudley has taken up one of the most difficult problems, and must not be contented with a partial investigation. It took some of us a long time to learn how

to make Bessemer steel at all; he must not expect to be able to so easily teach us how to make the best.

To illustrate why I object to his averages, I find among the rails taken from a level tangent that the one which shows the least loss in section and the least wear per million tons of traffic had carbon, 0·423; phosphorus, 0·127; silicon, 0·083; manganese, 0·708; while another rail presenting within three of the worst results, had carbon, 0·428; phosphorus, 0·109; silicon, 0·038; manganese, 0·870. The next poorest had carbon, 0·452; phosphorus, 0·144; silicon, 0·037, and manganese, 0·708, the manganese being exactly like the best. The poor steel had also very slightly the greatest density. The difference between 0·452 of carbon and 0·423 and 0·428 could easily be caused by rolling one steel at a higher heat than the other. Again, on the low side of level curves I find the second best rail had carbon, 0·454; phosphorus, 0·145; silicon, 0·015; manganese, 0·726, while the poorest rail had carbon, 0·497; phosphorus, 0·136; silicon, 0·062; manganese, 0·724. I cannot now believe that the slight difference in the chemical constituents of these rails caused the great difference in their wear. If such is the case, then I for one stand appalled at the difficulties which surround the making of a perfect rail.

As before stated, averages are dangerous. Dr. Dudley makes up a formula from the averages of his investigations, but admits that the silicon percentage is disturbed by an abnormal piece of steel, No. 881. This had carbon, 0·483; phosphorus, 0·035; silicon, 0·480; manganese, 0·782, and stands eighth in sixteen tests. It may be remembered that in the discussion at the Baltimore meeting in February, 1879, I mentioned a rail then in the track of the Boston and Albany Railroad that contained carbon, 0·360; phosphorus, 0·121; silicon, 0·469; manganese, 0·571, and which had then been in the track five years. That rail is still in service and in good condition. Here we have two rails made by widely-separated works, laid in the tracks of railroads hundreds of miles apart—one giving eleven years and one month of service, and then not worn out, but, on the contrary, selected as a "slow-wearing rail"; the other one good after over seven years of wear. Will we take the average composition of these rails which did not break, nor crush, nor rapidly wear out, and assume that carbon, 0·420; phosphorus, 0·079; silicon, 0·474, and manganese, 0·676, is the proper formula for rail steel? If not, why not?

Dr. Dudley's averages of his 32 samples of good wearing rails gave

him carbon, 0·334; phosphorus, 0·077; silicon, 0·060; manganese. 0·491. If that formula is right, stick to it. In fact, such a one will be better for both the producer and consumer than the compromise which Dr. Dudley recommends. I think every steel maker will bear me out in saying that sound steel can be more easily made under its provisions than with carbon from 0·25 to 0·35; phosphorus, 0·10; silicon, 0·04; manganese ranging from 0·30 to 0·40, aiming at 0·35. Theory is very fascinating, but in practice stubborn facts present themselves, and with steel containing 0·10 phosphorus, and not more than 0·35 manganese, the resulting ingots would be very unsound, and the rail-mill would produce an indefinite number of imperfect rails, many of which would get into service in defiance of the most careful inspection, the result being crushed ends, flat places, and generally unsatisfactory rails. If low manganese is desired the phosphorus must also be low; 0·10 per cent. cannot be so considered. In the 64 analyses there are but 16 with the manganese as low as 0·40 and under, and only 4 of these have the phosphorus above 0·085, 11 being under 0·07 and 6 under 0·05. The rails having less than 0·30 carbon, with the exception of six, were made over 12 years ago, and of these six, one was made 8 years, one 10 years and four about 11 years ago.

At the time all of these rails, excepting one, were made, all steel was hammered, the blooming-mill not having been invented. Under the hammer it is possible to coax steel into fair-appearing blooms that would either go to pieces, or roll very badly in the blooming-mill. When the latter was introduced the steel-makers had only at their command recarburizers poor in manganese and high in phosphorus. Moreover, the American irons were then even much higher in phosphorus than our chemists told us; hence, a great deal of very poor steel was made—by poor, I mean unsound steel. As high as 20 per cent. second quality or defective rails was a common run of work, while to-day, with better irons, richer spiegels and better melting-furnaces, we rarely exceed 1 per cent., and run for days at less than one-half of that figure, and this on much more difficult sections than railroad engineers formerly required.

Just here let me ask Dr. Dudley whether he gave sufficient consideration to this very difference of section. Examples are presented by rails 902 and 903. The first stands at the head of rails from the high side of curve-grade, and gave, upon analysis, carbon, 0·322; phosphorus, 0·077; silicon, 0·026; manganese, 0·492. The second had

carbon, 0·355; phosphorus, 0·108; silicon, 0·029; manganese, 0·490, and is at the foot of the list with but 2 years 11 months service, while the first had 7 years 11 months, and was of the old rounded head section, while the poor one, with almost the same chemical analysis, was of the square head pattern.

If, as suggested by Dr. Dudley, even softer rails are desirable, there need not be any difficulty in filling such an order. I would be perfectly willing to contract to make rails containing not over carbon, 0·15; phosphorus, 0·08, and manganese, 0·50. These rails would be perfectly homogeneous and stiffer than an iron rail of the same section, and ought, therefore, to hold up the load. The rail-makers, both in this country and England, are now making steel containing much more manganese than formerly. These rails are going out of the mills apparently better and sounder than those formerly made. Time only will demonstrate whether or not they are actually better.

The present Troy practice is to use irons containing the least phosphorus, to put in enough carbon to make strong steel, and enough manganese to make the steel roll sound, both while in the ingot and the bloom, to carefully heat the ingots and resulting blooms, hot straighten the rails so as to leave the minimum of work for the cold press, which does its work while the steel is yet hot. And we have to be yet convinced by the wear of our rails that this practice is wrong. For our tests we cast a 1-inch ingot from each blow; this is hammered into a ⅝ inch bar, which, when cold, is required to bend to at least a U by the blows of a sledge, this bending being a much severer test than when done in a press. The steel is also quenched in water and tested for temper. Drillings are taken from the ingot, and *accurate* carbon determinations made.

I prefer this plan to any test of the rail ends. To be perfectly conclusive, such tests would have to be made of both ends of the rail, and from every rail, for one end might be overheated and the other not. Some blooms might be all right, and the rest of them heat-spoilt. In a mill producing, say 9000 rails per week, 18,000 rail-ends tests would be no inconsiderable item, particularly if, as Dr. Dudley proposes, the test-piece, "12 inches long, 1½ inch wide, ½ inch thick," is to be slotted from the web of the rail. He would have to give a lease of the Altoona shops along with the rail contract.

Dr. Dudley's theory of infinitesimal teeth is interesting, and, if true,

I should prefer having the teeth of my rack so strong that they would neither break off or flatten down.

As a matter of perhaps some interest, I present two pieces of Troy rails, cut off at the saws from two rails, not in the same heat, and tested without knowing anything of their chemical composition. I had these pieces separately placed upon 10-inch bearings under a 7-gross ton hammer, a piece of $2\frac{1}{2}$-inch round iron laid upon them as a fuller, and the hammer allowed to fall from twenty inches above the fuller, which, according to Haswell, gave a blow of 67·75 gross tons. The pieces were then turned over, the fuller placed upon the convex surface and the hammer allowed to fall from 13 inches above the fuller, giving a blow of 58·45 gross tons. You will see that the rails do not show any signs of rupture, and their color at the points of torture prove them to have been absolutely cold when the test was made. I think these rails ought to be reasonably safe in the track. As you see by this piece of the head of one of these rails, I had it planed, and then some teeth cut in it by a cold chisel, and one-half of them pounded down with a hammer. The teeth of my rack did not break off. The analyses of these rails subsequently made are:

	I.	II.
Carbon,	0·410	0·380
Silicon,	0·050	0·058
Phosphorus,	0·086	0·082
Manganese,	0·942	0·840

In conclusion, I will say that Dr. Dudley's paper as a contribution to our knowledge of steel rails is valuable and interesting, but I protest against his conclusions being received as manufacturing or commercial axioms. From its being presented in the form of a report to the leading railroad of the country from one of its trusted officers, as well as from the tone of the concluding deductions, it is liable to be so received by railroad men. If any railroad company desires rails made under either of Dr. Dudley's formulas, and are willing to pay a price large enough to cover the loss in making them, well and good; but so long as the rail makers are compelled to *guarantee* the wear of their rails for a given number of years, justice requires that the composition of the steel from which these rails are made should be left to them.

William Sellers, Philadelphia.—The very interesting paper upon the wear of steel rails that has just been read presents the record of a series of investigations that are extremely valuable, and the deduction that has been drawn from the results noted seems to be unavoidable; the tests, however, to which it is proposed steel rails shall be subjected hereafter, with a view to determine their quality, should have our careful attention, and with reference to these I desire to make a few suggestions.

While the manufacture of soft steel was yet in its infancy it was believed that the presence of phosphorus in any notable quantity was very deleterious, in fact fatal, to the good quality of this product, and the greatest care was exercised to procure materials in which this element could not be more than traced. With the development of this art it has been found that larger and larger proportions of this hurtful ingredient may be used, providing always corresponding changes in the chemical composition shall be made to accord therewith.

It is, perhaps, besides the point to inquire whether the earlier belief was correct or not, the fact remains that steel is now produced which contains much larger proportions of phosphorus than would have been permitted a very few years ago, and that this steel is now considered to possess qualities which fit it admirably for use in rails; moreover, it is well known that the degree of heat, and the manipulation to which the ingot is subjected in transforming it into the finished product has an important influence in determining the characteristics that product will exhibit.

These facts have an important bearing upon the question, What shall be the tests which are to determine the quality we desire to attain? If the engineer is to specify the chemical composition, and the mode or process of manufacture by which the manufacturer must work, it would seem that improvement in the art must, to a certain extent, be limited, and a vicious system would be introduced, injurious alike to the engineer and the manufacturer. The chemical composition has no value to the engineer, for no matter what the chemical composition, it is upon the physical qualities at last that he must rely to determine whether or not it will answer his purpose. It should be his business, therefore, to devise such physical tests as will determine absolutely whether or not the quality that he desires has been produced, while the manufacturer should be left free to make such chemical combinations or adopt such processes of manufacture as will fulfill the requirements of

the engineer. It may, however, be questioned whether the physical data we now possess will enable us to agree upon the physical tests requisite to demonstrate the quality; but if this is admitted it only proves that further physical data are wanting, for the quality is finally determined by use, the result of which our physical data should enable us to predict. Of these data the one most abundant is the ultimate strength, and next to this ductility, bending, shearing, punching, torsion, impact and fatigue, in all of which, except the last, abundant facts are at hand. As most specifications prescribe a high and a low limit for ultimate strength, it would seem to be the prevalent opinion among engineers that high or low steel, as it is technically termed, is to be determined by its ultimate strength; and it becomes important at this stage to define what quality it is that most accurately defines this term, that is to say, does it consist in high or low ultimate strength, or in high or low ductility relatively to its ultimate strength? It is essential for all structural uses that the engineer should know upon what ultimate strength he can rely, for upon this all his calculations must be based; but it is upon ductility that he must depend for safety, the measure of which he must determine, after which the higher the ultimate strength he can obtain the better his material will be for any structural purpose; that is to say, with a given ductility the higher the ultimate strength of his material the safer it is, and conversely, with a given ultimate strength, the higher the ductility the safer it is. High ductility and high ultimate strength cannot be produced except with the most favorable conditions, both as to chemical composition and as to the mode of manufacture; the quality is, therefore, to be ascertained with most certainty by determining the relation of the one to the other, and for the same reason this relation would seem to be the factor which should most accurately define the term high or low, as applied to steel, and the requirements simply of an ultimate strength not less than ——— pounds per square inch, with a ductility not less than ——— per cent., would at once determine the character of steel required and the quality of it. It must not be understood, however that the determination of this relation is the sole requisite in determining quality for every purpose, for the capacity to bear fatigue and shock is scarcely less important, as, for example, the question now under consideration. And although it is probable that the material which exhibits high ultimate strength coupled with high ductility will prove to be capable of enduring the most fatigue and shock,

we cannot affirm that there is any definite relation between the two, in fact we have many data tending to show that such a relation does not exist.

An examination of the data which Dr. Dudley has tabulated indicates that to establish the relation between ultimate strength and ductility alone would be insufficient to determine the wearing quality of rails, so that these data must be supplemented by some other. This other, I suggest, should be that of fatigue from shock, not that of simply bending, which last Dr. Dudley "has found to bear a closer relation to the loss of metal per million tons than any of the other tests." I take exception to the classification of this test as one of the four ways in which a bending test could be applied. A rail bent under the drop test, and one bent in the testing machine by pressure slowly applied, would not be subjected to the same character of strains. While a drop test is a bending test, it is also much more: the same number of degrees of deflection in the one case as in the other would, I think, represent very different powers of resistance in the material operated upon. With the same weight falling from the same height in properly constructed guides, the same *effect* must be produced with every blow. In fact, it is difficult to conceive how any other form of test can produce more uniform effects or which can be more accurately measured; the *results* may be more diverse with such a test than with others, because they are produced by pressure and shock, whereas nearly all other forms of testing produce their results by pressure alone. It is this difference, however, which commends the drop as the test above all others for rails as being more analogous to that by which the rail is tested in use, and we should be well satisfied that the objections urged against it are well founded before we abandon it for others which may appear to offer more uniform results. It may well be that uniform results obtained by a system of testing widely different from that we require our material to sustain in use may have small value for determining in advance the effect of that use. I am thus driven to the conclusion that to obtain the relation of ductility to ultimate strength, together with the capacity to sustain fatigue from shock, would be to attain to absolute certainty as to the quality in an engineering sense.

There are, however, considerations other than that of the tests which must have attention before adopting a system of testing for steel rails, and as to these I would now make a few suggestions. The tests

required to determine quality in the directions indicated are well understood, but, simple as they are, the time that must be consumed in making them would result in serious loss to the manufacturer if his mill is to be held for their determination, and if he proceeds with the execution of his order in advance he incurs a serious responsibility in assuming the risk of rejection for the large product that would be turned out before the requisite tests could be made. While the cost of a test for ultimate strength, ductility and for capacity to bear fatigue would be small, the large number of such tests that would be necessary to establish the quality of an ordinary order for steel rails would be a serious item, and as every item of cost must be eventually borne by the consumer, it is important for the railway companies to adopt a regular system for testing their rails that shall not only be the most expeditious, but the least expensive. For the purpose of inspection, therefore, it would seem to be sufficient to adopt a system that would be simply an indication as to the qualities desired, without subjecting the parties interested to the cost and delay which must result from exhaustive and thorough tests, and upon these indications the rails might be accepted. This would seem to accord with the best foreign practice, as illustrated by the very admirable paper upon "Rail Specifications and Rail Inspection in Europe," by C. P. Sandberg, C.E., read at the Lake Superior Meeting, August, 1880. There are two tests which would give these indications with great accuracy, both of which could be applied without the expense and delay incident to preparation of specimens, and both of which require comparatively inexpensive machinery. These are the registering punch and the drop test. The former is a special tool which could be applied upon the crop ends, and could be portable; the latter is too well known to require description, but its indications if carefully noted would, I believe, be the most valuable of the two, but in conjunction with the punch they should be conclusive. The punching test would have this advantage: that by its use an inexpensive indication could be had of the quality of every rail. The suggestion that this test should be applied upon the fish-plate holes has probably prevented its introduction heretofore; first, because such holes are not now punched, and second, because to make such registration every manufacturer would have to procure a registering punch, and this would be difficult to apply upon existing machines. If this tool should be recognized as a part of the inspector's outfit and

specially adapted to his needs, it might soon come into general use if care was used to maintain the punches and dies in good condition.

In conclusion, I suggest that if the physical tests are to be supplemented by chemical analysis the specification for this analysis should not be complete; that is to say, in place of giving the proportions of carbon, phosphorus, silicon and manganese, a maximum limit should be fixed respectively for phosphorus, silicon and manganese only, leaving the carbon to be varied by the manufacturer, so that he may properly be required to furnish material that will fulfill the physical conditions; for it is evident that if the engineer defines the chemical composition he cannot reasonably ask the manufacturer to gurantee that this composition shall give certain physical results.

W. R. JONES, Pittsburgh, Pa.—The question that naturally occurs to me is this: Has Dr. Dudley in his investigations been aiming to prove a theory, or has he been guided by an earnest desire to discover what are the proper elements in the composition of a good-wearing steel rail?

Unfortunately, for correct chemical information, he has omitted in his analyses two very important elements—sulphur and copper. Now, before we will even begin to admit the correctness of Dr. Dudley's conclusions and the formula he prescribes, we will at the start question the propriety of any chemist or scientist prescribing a formula for making steel when he has ignored such important elements as sulphur and copper. I, for one, will not accept any such formula.

Are we sure, or is Dr. Dudley sure, that the chemical analyses embodied in his paper are correct? This may seem a presumptuous question; yet, with my experience with chemists, I naturally doubt the correctness of the analyses, and, before I will accept them as correct, I will ask that comparative tests of phosphorus and manganese be made by the Pennsylvania Railroad chemists and the chemists of the leading steel-works in the country. Let us first verify the correctness of the analyses before we consider the conclusions. I can enumerate a great number of instances in which chemists have differed very widely in their determinations of phosphorus and manganese. A prominent iron firm made a contract with the Edgar Thomson Steel Company to deliver pig metal guaranteed to be between 0·07 and 0·08 phosphorus. An analysis by our chemist resulted in phosphorus 0·148 and 0·152—a rather startling difference! Again, a sample bar

of steel, in which our chemist reported phosphorus 0·11, was tested by a chemist of another Bessemer works, and his determinations were phosphorus between 0·07 and 0·08. A leading engineering establishment of Pittsburg bought iron claimed by a chemical analysis to contain 0·03 phosphorus; our Mr. Ford found phosphorus 0·145. A chemist connected with an open-hearth works reported manganese in a piece of steel, 1·14; in a second determination from the same piece of steel, by the same chemist, manganese was reported 0·43. The chemist was kept in ignorance of the fact that both samples were from the same piece of steel. Two determinations for manganese were made by the same chemist from a piece of steel; he reported manganese 0·61 and 0·58. Our chemist, in the same steel, reported manganese 0·324 and 0·303. I could give innumerable instances of the wide difference in chemists' determinations of phosphorus and manganese. I think I have cited sufficient cases to sustain the position I have assumed, viz., that before the determinations made by Dr. Dudley's assistants are accepted as being correct they should be verified by chemists of greater experience.

I find on a close examination of the doctor's paper that he has taken no notice whatever of the increased weight of cars, increased weight of locomotives, with increased speed, in his tonnage calculations. Now, there is a vast difference between the tonnage of 10 to 12 tons in an ordinary freight-car with eight wheels passing over rails at a moderate speed, and 15 to 20 tons on the same number of wheels at an increased speed. Since 1874 the Pennsylvania Railroad Company has been steadily increasing the weight of both engines and cars. The duty to which rails are now subjected, I believe, is fully 60 per cent. greater than that before the year 1874. On looking over the paper, we find a number of rails, classed as good-wearing rails, that have for years been subjected to comparatively light tonnage on a wheel-tonnage basis, compared with a great number of rails classed as fast-wearing rails, which in some cases I find have had passing over them nearly twice the number of tons per month, and all on heavier wheel tonnage.

As an illustration, I cite rail No. 937, with the following analysis: carbon, 0·454; silicon, 0·015; phosphorus, 0·145; manganese, 0·726, with only 2 per cent. elongation. In accordance with the deductions and formula this should be a very bad rail, yet on close examination I find that this rail has been subjected to a monthly tonnage of 747,628

tons. If we examine the rail No. 929, which was laid within two miles of rail 937, we find carbon, 0·235; silicon, 0·080; phosphorus, 0·055; manganese, 0·300; elongation, 24 per cent. This rail was subjected to a monthly tonnage of only 381,235 tons, while rail No. 937 was subjected to 96 per cent. more monthly tonnage, and yet rail No. 929 is classed as a good rail. If we assume that 50 per cent. more loaded cars pass east to Philadelphia than pass west from Philadelphia, we find the wheel-tonnage assumes a very important aspect in determining the wearing qualities of rails. Again, the bad rail was in the track 40 months, and only shows a wear of $\frac{1}{100}$ of an inch in vertical section, and has been in the track since the advent of heavier engines and heavier cars, while rail No. 929 has had the advantage of at least 7 years of comparatively light traffic on light-wheel tonnage. The question, which of these two rails has been subjected to the greatest amount of wheel-tonnage, I leave to some one to calculate who has more time to devote to this subject than I have.

In regard to the method proposed by Dr. Dudley to test the rails at the works, I can only say I much prefer the methods suggested by Mr. Sandberg, in his paper read before the Institute at the Lake Superior meeting, with this slight modification, viz.: I would subject a 50 and 52 pound rail to a drop-test of 1800 pounds, falling a distance of 14 feet on the rail, on supports 3 feet apart; for a 54 to 56 pound rail, 16 feet drop; for a 56 to 58 pound rail, 18 feet drop; for a 58 to 60 pound rail, 20 feet drop, and so on in the same ratio. I would also adhere to the test-bar, drawn out from the head of the rail down to one inch square, then placed under a steam-hammer and bent through an angle of 110°, the distance between centres of supports of the bar to be from 10 to 12 inches. Dr. Dudley may think these tests crude; I believe them to be simple, thorough, effective and reliable, and in this I fully concur in the views of Mr. Sandberg.

If Dr. Dudley and the Pennsylvania Railroad authorities believe their deductions are correct, let them have rails made in accordance with the doctor's first formula—phosphorus 0.077, carbon 0·334, silicon 0·060, manganese 0·491—and add to it the less sulphur and copper the better, and, as a matter of course, pay the difference in price involved in the difference in the price of metals; but when Dr. Dudley attempts to formulate a rule to govern the steel-makers, based on his knowledge, I for one decidedly object, and I frankly tell him that

he is opposing all the researches and investigations of the best chemists and metallurgists, both here and abroad.

I have serious and grave doubts if steel made in accordance with his second formula would give a good record in the track. I experimented on this formula in attempting to fill an order. Mr. Sandberg in his paper refers to the filling of an order of 2500 tons on the same formula, and my experience was the same as his. The ingot was a conglomerate mass of honeycombs. It made bad blooms, and I do not believe it made good rails. The rails are now in the tracks of the West Pennsylvania road, and if they do prove to be good rails I shall be very much surprised.

To be continued.

AN ACCOUNT OF EXPERIMENTS MADE BY A BOARD OF UNITED STATES NAVAL ENGINEERS WITH SCREW PROPELLERS OF DIFFERENT MATERIAL AND DIMENSIONS, APPLIED TO THE UNITED STATES FISH COMMISSION'S STEAMER "LOOKOUT," WITH THE HULL COPPERED AND NOT COPPERED.

By Chief-Engineer ISHERWOOD, U. S. Navy.

In the refitting, at the Washington, D. C., navy-yard, of the United States Fish Commission steamer *Lookout*, seven screws were adapted to her, differing in diameter, pitch and fraction of the pitch used, with the view of experimentally ascertaining the best proportions of these for that particular vessel. Two of the screws being of cast iron and the remaining five of cast brass, they furnished an opportunity for observing the comparative effect of these two metals upon the economic efficiency of the screws. Further, as the vessel's bottom was covered with rolled copper during the experiments with two of the screws, while it was not so covered during those with the remaining five screws, but exposed to the water only the wooden surface of a vessel in the usual condition for coppering, the occasion was afforded to determine the effect of such coppering or want of coppering upon the resistance of the hull.

The experiments with these screws were made by a Board of Chief-Engineers of the United States Navy, which reported the data to the Bureau of Steam-Engineering of the Navy Department. The writer has taken these data, corrected and re-arranged them, and from them has made the calculations and deductions that will be found in this paper, none of which are in the Board's report.

The experimental deductions contain two novelties, namely, the effect upon the economic efficiency of screws due to their being constructed of cast brass or of cast iron, and the effect upon the resistance of wooden hulls due to having their bottom coppered or of presenting to the water a wooden surface in the usual state preparatory to receiving the copper; or, in other words, the determination of the comparative resistance to motion in water of hulls having their immersed wetted surface of copper or of planed wood. In the absence of other experiments on these points, the present ones will be of interest, taking care to discriminate that the comparison holds strictly true only for the exact experimental conditions, any variation of which would consequently vary the comparative results; but, although the experiments determine these results quantitatively for only the particular cases on trial, yet they remain qualitatively true for all cases.

Taking the resistance of a hull as composed of the resistance of its immersed wetted surface to the water; and of the resistance of the water to displacement by it, that is to say, the resistance of water to being elevated from the centre of gravity of the greatest immersed transverse section of the hull to whatever height above the water-level it may be forced by the progress of the vessel; there follows that the ratio of these two resistances will vary according to the form or model of the hull. If the hull, of given lineal dimensions, be very sharp, the larger portion of its resistance as a whole, will be composed of its skin resistance or that of its immersed wetted surface to the water, while the remaining smaller portion is composed of the resistance of the water to displacement. And, *vice versa*, if the hull be of very full form, the larger portion of its resistance as a whole will consist of the resistance of the water to displacement, while only the remaining smaller portion is due to the skin or wetted surface resistance. In the case of the *Lookout*, the hull was very sharp, exposing a great extent of immersed wetted surface comparably to its displacement; consequently the difference between the resistances of the two kinds of surface—rolled copper and planed wood—will be proportionally more

marked in her than in the cases of vessels with fuller forms. However hard and smooth a wooden surface may be made out of water, yet when immersed it will become water-soaked and soft, the small exposed fibres of the wood separating, rising and forming a delicate hairy coating, offering much more resistance to water than the original hard plane surface from which they arose.

It is to be regretted that the experiments were so few: they might easily have been extended by cutting off two of the four blades of the screws, by successively reducing their diameters and lengths, etc., which modifications could have been executed at little cost as the screws did not exceed 5 feet in diameter, a size readily handled yet large enough for reliable results; but, as the vessel was not entirely at the command of the Bureau of Steam-Engineering, only such trials were available as could be made without interfering with her other uses.

The experiments do not show any superiority of cast brass over cast iron for the material of the screw; from which may be inferred that, per unit of surface moving at equal speed, the resistance of the water to the cast iron surface is no greater than to the cast brass surface. Also, that the less direct resistance of the thinner cutting or forward edges of the blades of the brass screw was not sufficient to produce a sensible effect on the economic result. The much less cohesive strength of cast iron than of cast brass requires the blades of screws made of the former metal to be much thicker for equal strength than those made of the latter metal; and in the case of the experimental screws, the cast iron blades were about two and a half times thicker than the cast brass ones. Of course, the thicker blades must be attended with more direct resistance than the thinner ones, but the edges, forward and aft, of all blades are so acutely beveled that any difference due to this cause seems to be too slight to affect the result in a marked manner.

Hull.

The *Lookout* was constructed of wood for a steam yacht, and had a light schooner rig. The bottom was uncoppered during the trials with screws *A*, *B*, *C*, *D* and *E*; but it was coppered during the remaining trials with screws *F* and *G*.

The following are the dimensions and proportions of the hull cor-

responding to the draught of water which was maintained without sensible variation during all the trials:

Length on water-line from forward edge of rabbet of stem to after side of the sternpost,		96 feet.
Greatest breadth on water-line,		13 ft. 6 in.
Extreme breadth,		16 feet.
Draught of water from bottom of keel,	Forward,	2 ft. 9 in.
	Mean,	3 ft. 8 in.
	Aft,	4 ft. 7 in.
Depth of keel below lower edge of its rabbet,	Forward,	6 in.
	Mean,	1 ft. 2 in.
	Aft,	1 ft. 10 in.
Mean depth of hull from water-line to lower edge of rabbet of keel,		2 ft. 6 in.
Area of the greatest immersed transverse section,		25·28 sq. ft.
Area of the water-line,		861·44 sq. ft.
Displacement (35 cubic feet per ton),		42·87 tons.
Displacement per inch of draught at water-line,		2·051 tons.
Ratio of length to breadth on water-line,		7·1111
Ratio of the greatest immersed transverse section to its circumscribing parallelogram,		0·7490
Ratio of the water-line to its circumscribing parallelogram,		0·6647
Ratio of the displacement to its circumscribing parallelopipedon,		0·4631

ENGINE.

The vessel had one vertical, direct-acting, compound, condensing steam engine, with the two cylinders placed side by side and acting on cranks at right angles to each other.

Diameter of the small cylinder,	12 inches.
Diameter of piston rod of the small cylinder,	1½ inches.
Net area of the piston of the small cylinder,	112·2140 sq. in.
Stroke of the piston of the small cylinder,	16 inches.
Space displacement of the piston of the small cylinder, per stroke,	1·0390 cu. ft.
Diameter of the large cylinder,	20 inches.
Diameter of the piston rod of the large cylinder,	1½ inches.
Net area of the piston of the large cylinder,	313·2764 sq. in.

Stroke of the piston of the large cylinder, .	16 inches.
Space displacement of the piston of the large cylinder, per stroke, . . .	2·9007 cu. ft.
Ratio of the space displacement of the piston of the large cylinder, per stroke, to that of the piston of the small cylinder, .	2·97178

Boiler.

There was one cylindrical boiler, with cylindrical furnaces, and horizontal fire tubes returned over them. A large portion of the steam room was in a steam-chimney or drum placed above the shell and traversed by the chimney, so that the steam in the drum was, in a measure, superheated by the escaping hot gases of combustion.

Diameter of the boiler shell, . .	7 feet.
Length of the boiler shell, . .	8 feet.
Diameter of the steam-chimney, . .	4 ft. 6 in.
Height of the steam-chimney, . .	5 ft. 6 in.
Number of furnaces, . . .	2
Diameter of the furnaces, . .	2 ft. 4 in.
Length of the grates, . . .	5 feet.
Area of grate surface, . . .	$23\frac{1}{3}$ sq. feet.
Area of water-heating surface, calculated for outside of tubes, . . .	519 sq. feet.
Calorimeter, or aggregate cross area of tubes, for draught, ...	3·14 sq. feet.
Steam room in boiler shell and in steam-chimney,	94·54 cu. feet.
Square feet of water-heating surface per square foot of grate surface, . . .	22·2429
Square feet of grate surface per square foot of cross area of tubes, . . .	7·4310

Screws.

The screws experimented with were seven in number, and are designated by letters from *A* to *G*, both inclusive. In the tables hereinafter given, the columns headed with these letters contain the data and results of the experiments made with the screws of corresponding letters.

All the screws were of cast brass with the exception of screws *B*

and *G* which were of cast iron and had blades about two and a half times thicker than the brass ones.

All the screws had four blades and a uniform pitch, which varied for the different screws from 7·50 to 9·08 feet. The mean fraction used of the pitch varied also from 0·1994 to 0·5736. The diameter of all the screws, except *A* and *F*, was 5 feet; that of *A* was 4·6458 feet and that of *F* was $4\frac{2}{3}$ feet.

When the vessel was at rest, a small portion of the 5 feet diameter screws protruded above the water; but when the vessel was in motion at the experimental speeds, all the screws were submerged, owing to the dropping of the stern.

The following table shows the principal dimensions of the screws:

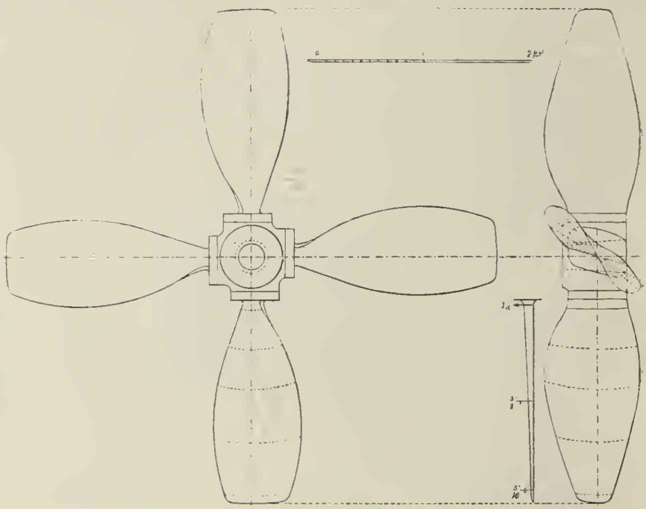
Designation of the screws.	Diameter, feet.	Pitch, feet.	No. of blades.	Greatest length of screw in direction of axis. Feet.	Projected area of blades on a plane at right angles to axis. Square feet.	Helicoidal area of blades. Square feet.	Mean fraction used of the pitch.
A	4·6458	8·400	4	0·9167	5·4551	7·9447	0·3325
B	5·0000	7·500	4	1·1875	10·1693	13·5266	0·5283
C	5·0000	7·500	4	0·5208	5·2457	6·7860	0·2703
D	5·0000	8·625	4	0·4461	3·8697	5·3470	0·1994
E	5·0000	9·000	4	0·8125	6·8975	9·3407	0·3549
F	4·6667	9·000	4	0.8125	5·9659	8·6850	0·3539
G	5·0000	7·900	4	1·2708	11·0098	14·8453	0·5736

The following are detailed descriptions of each screw, wherein the form of the blade, or its outline, is given, together with its thickness and the diameter of the hub, etc.

Screw *A*.

This screw was of cast brass and had a uniform pitch. The blades, when viewed in projection on a plane parallel with the axis, increased

gradually in length from the hub to a radius of $13\frac{1}{2}$ inches, from which their length gradually decreased to the periphery. The hub was square and the blades—which were cast separately—were bolted to it. The thickness of the blades at the fillet of the hub was $1\frac{1}{16}$ inches.

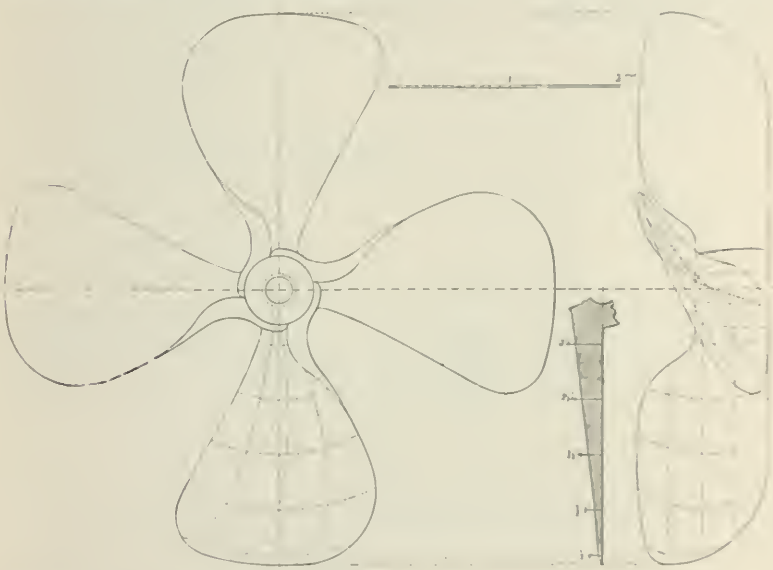


Diameter of the screw, . . .	4·6458 feet.
Diameter of the hub, . . .	0·8333 feet.
Pitch of the screw, ...	8·4 feet.
Number of blades, . . .	4.
Length of the screw at the hub, in the direction of the axis, ...	0·5625 foot.
Length of the screw at radius of $13\frac{1}{2}$ inches, in the direction of the axis, . . .	0·9167 foot.
Length of the screw at the periphery, in the direction of the axis, ...	0·3125 foot.
Mean fraction used of the pitch, . .	0·3325
Projected area of the blades on a plane at right angles to the axis, . . .	5·4551 sq. ft.
Helicoidal area of the blades, . .	7·9447 sq. ft.

Screw *B*.

This screw was of cast iron, and had a uniform pitch. The hub and blades were cast together. The blades, when viewed in projec-

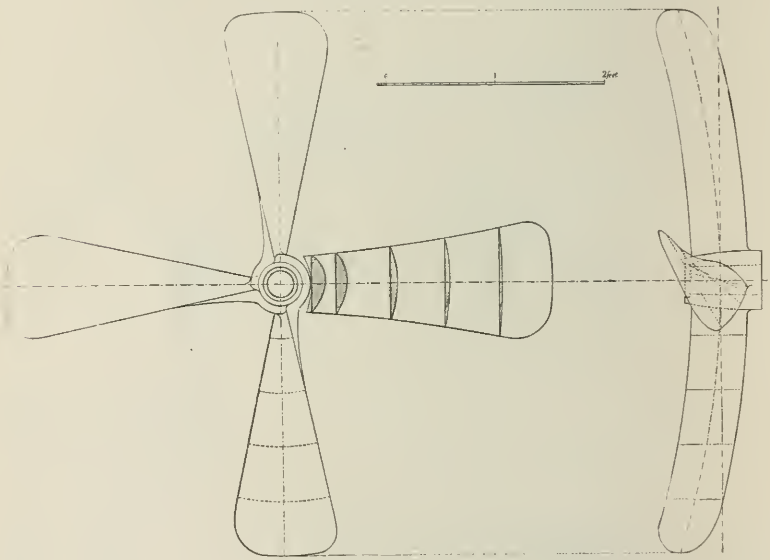
tion on a plane parallel with the axis, had their edges parallel from the hub to a radius of 6½ inches. From this point to a radius of 18 inches the length of the blades gradually but irregularly increased. From the radius of 18 inches to the periphery the length of the blades gradually decreased. The outer corners of the blades were excessively rounded. The hub was cylindrical, and the thickness of the blade at its fillet was 3 inches.



Diameter of the screw, . . .	5·00	feet.
Diameter of the hub, . . .	0·70	foot.
Pitch of the screw, ...	7·5	feet.
Number of blades, . . .	4.	
Length of the screw at the hub, and to radius of 6½ inches, in the direction of the axis, . .	0·6667	foot.
Length of the screw at radius of 18 inches, in the direction of the axis, . . .	1·1875	feet.
Length of the screw at the periphery, in the direction of the axis, ...	0·5080	foot.
Mean fraction used of the pitch, . .	0·5283	
Projected area of the blades on a plane at right angles to the axis, ...	10·1693	sq. ft.
Helicoidal area of the blades, . .	13·5266	sq. ft.

Screw C.

This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{4}$ inches.



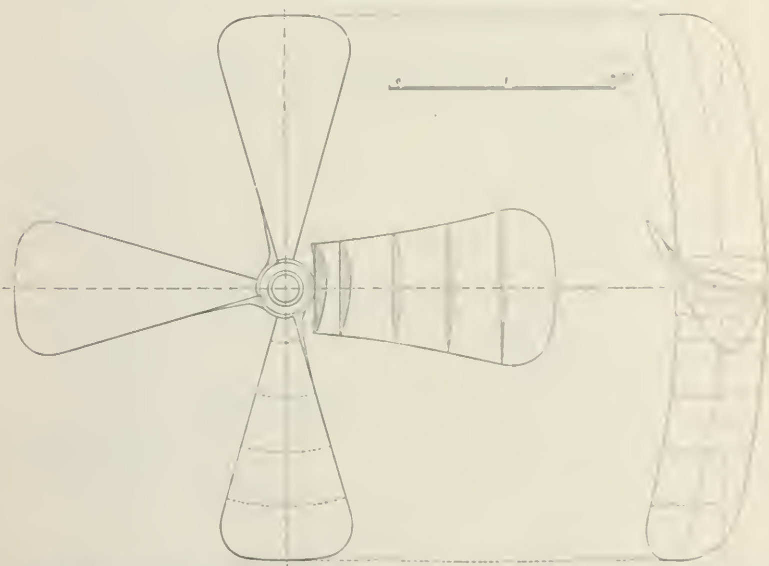
Diameter of the screw, ...	5·0 feet.
Diameter of the hub, . . .	0·5417 foot.
Pitch of the screw, ...	7·5 feet.
Number of blades, . . .	4.
Length of the screw in the direction of the axis, uniform from hub to periphery, . .	0·5208 foot.
Fraction used of the pitch, . . .	0·2703
Projected area of the blades on a plane at right angles to the axis, . . .	5·2457 sq. ft.
Helicoidal area of the blades, . .	6·7860 sq. ft.

Screw *D*.

This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{4}$ inches.

Diameter of the screw, . . .	5·000 feet.
Diameter of the hub, . . .	0·5417 foot.
Pitch of the screw, ...	8·625 feet.
Number of blades, . . .	4.
Length of the screw in the direction of the axis, uniform from hub to periphery, . .	0·4461 foot.
Fraction used of the pitch, . . .	0·1994
Projected area of the blades on a plane at right angles to the axis, . . .	3·8697 sq. ft.
Helicoidal area of the blades, . .	5·3470 sq. ft.

Screw *E*.



This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed

in projection on a plane parallel with the axis, were parallel segments of circles, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{8}$ inches.

Diameter of the screw, . . .	5· feet.
Diameter of the hub, . . .	0·5625 foot.
Pitch of the screw, ...	9· feet.
Number of blades, . . .	4.
Length of the screw in the direction of the axis, from hub to periphery, . . .	0·8125 foot.
Fraction used of the pitch, . .	0·3549
Projected area of the blades on a plane at right angles to axis, . . .	6·8975 sq. ft.
Helicoidal area of the blades, . . .	9·3407 sq. ft.

Screw *F*.

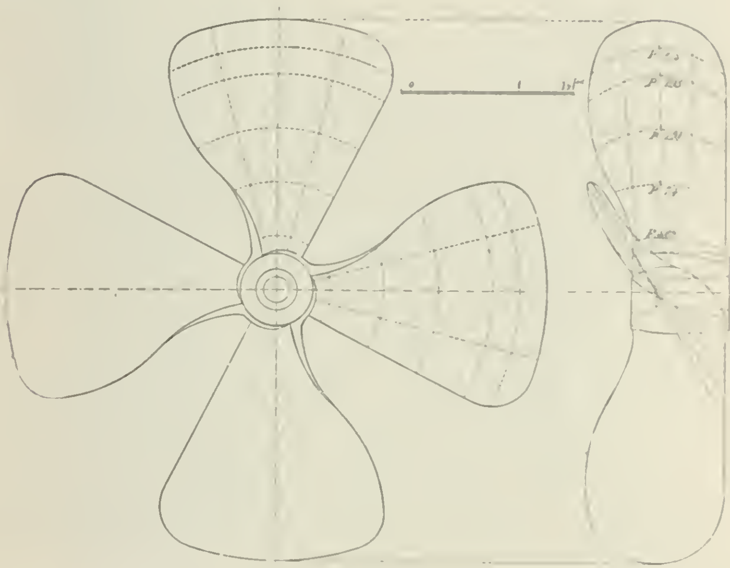
This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{8}$ inches. In all respects except diameter, screw *F* was the duplicate of screw *E*.

Diameter of the screw, . . .	$4\frac{2}{3}$ feet.
Diameter of the hub, . . .	0·5625 foot.
Pitch of the screw, ...	9· feet.
Number of blades, . . .	4.
Length of the screw in the direction of the axis, from hub to periphery, . .	0·8125 foot.
Fraction used of the pitch, . . .	0·3539
Projected area of the blades on a plane at right angles to axis, . . .	5·9659 sq. ft.
Helicoidal area of the blades, . . .	8·6850 sq. ft.

Screw *G*.

This screw was of cast iron, and had a uniform pitch. The blades, when viewed in projection on a plane parallel with the axis, increased gradually in length from the hub to a radius of 21 inches, from

which the length gradually decreased to the periphery. The hub was cylindrical, and the thickness of the blade at its fillet was 4 inches.



Diameter of the screw, . . .	5·0	feet.
Diameter of the hub, . . .	0·75	foot.
Pitch of the screw, ...	7·9	feet.
Number of blades, . . .	4.	
Length of the screw at the hub, in the direction of the axis, ...	0·8542	foot.
Length of the screw at radius of 21 inches, in the direction of the axis, . . .	1·2708	feet.
Length of the screw at the periphery, in the direction of the axis, ...	0·8750	foot.
Mean fraction used of the pitch, . .	0·5736	foot.
Projected area of the blades on a plane at right angles to the axis, . . .	11·0098	sq. ft.
Helicoidal area of the blades, . . .	14·8453	sq. ft.

Experiments.

The experiments were made in the perfectly smooth water of the Potomac River near the city of Washington, over a base of 11⅞ geo-

graphical miles, according to the Coast Survey Chart, extending from marks abreast the wharf at Giesborough Point to marks abreast the wharf at Marshall Hall. During all the experiments, the position of the throttle and of the cut-off valves remained the same, nor was there any sensible change in the general state of the machinery. The steam pressure in the boiler was maintained as nearly as possible at 80 pounds per square inch above the atmosphere, and the engine and boiler were managed throughout by the same persons.

Each experiment was made in exactly the same manner. Steam was raised and the machinery operated at the Navy Yard wharf until the fires were brought to steady action, when the vessel proceeded to the nearest terminus of the base, which she passed at her maximum speed for the boiler pressure, when one observer took the time and another the reading of the taffrail log made by Bliss & Co. As the vessel passed the farthest terminus the time and log reading were again taken. The vessel was not put about until at a distance beyond the last terminus that would allow her on the return to pass it at the maximum speed for the boiler pressure, as before. The reading of the engine counter in the engine room was taken simultaneously with the log and time.

The indicators employed were new, and of Thompson's design; during each run, excepting the first four with screw *A*, there were taken by them six sets of indicator diagrams from each end of each cylinder, the diagrams being equispaced over the time. The indicated pressures in the different experiments are the means of all the diagrams taken. During only the last two runs with screw *A* were there any diagrams taken, and the mean pressure from them has been assumed as the mean pressure for the entire experiment.

During all the runs the vessel's trim and draught of water remained sensibly the same.

The experiments with screws *A, B, C, D* and *E* consisted of six runs over the base with each, three in each direction, and they were made on three different days, a pair of runs being made in immediate succession on each day. The experiments with screws *F* and *G* were made in the same manner, except that they consisted of only four runs over the base, two in each direction, and they were made on two different days, a pair of runs being made in immediate succession on each day.

During experiments *A, B, C, D* and *E* the vessel was not coppered,

the immersed portion of the hull presenting a planed wooden surface to the water. During experiments *F* and *G* this portion was covered with smooth rolled copper.

The speed of the vessel was determined, as stated, by the shore distances and by the taffrail log for each run over the base. The mean of the speeds of the different runs of the same experiment, by the shore marks, is taken as the mean for the entire experiment according to that measurement. And, in the same manner, the mean of the speeds of the different runs of the same experiment by the taffrail log is taken as the mean for the entire experiment according to that measurement. These two measurements differed, and, with a slight irregularity for the different experiments, the mean difference deduced from all the experiments showing, as might be expected, that the indications by the taffrail log were greater than those by the shore marks, the ratio being as 1·0572 to 1·0000. This ratio may be taken as a constant correction for the taffrail log, which, as its determinations were unaffected by current, can be considered, when thus corrected, as the most accurate measure of the vessel's speed. Accordingly, the speed by the log thus corrected is what is given in the following tables.

The mean number of revolutions made by the screw per minute during a run was ascertained from the time and the total number of revolutions given by the counter during that run; and the mean number of revolutions made per minute by the screw during an experiment is the mean of the means of those made during the runs composing that experiment.

[To be continued.]

Volatility of Sulphuric Acid.—Regnault has shown that mercury is volatile even at very low temperatures; but it has been generally thought that sulphuric acid is not volatile at ordinary temperatures. A writer in *Il Progresso* has found that in operating in an atmosphere that had been dried with sulphuric acid, litmus paper was soon discolored and gradually disorganized. This fact may explain the sulphur rays which are often observed in Geissler tubes. It may also be of great importance in chemical analyses, where the vapor of sulphuric acid under certain conditions might change the character of compounds without its presence being suspected.—*Chron. Indust.* C.

PERCUSSION ROCK DRILLS.

By Robert Grimshaw.

Paper read before the Franklin Institute June 15, 1881.

The name "rock drill" is in most cases a misnomer, as the devices used for perforating rock very seldom have a drilling action proper. The French word "*perforatrice*," or perforator, is used with better reason. However, there is little more use in calling attention to this fact, than in stating our use of the word fore-plane is incorrect and the English use correct; the English meaning by fore-plane what we call a jack-plane, which is really the *before* plane.

As ordinarily known, a drill is a device which, by rotation and lengthwise advance, makes a round hole in some solid material, as wood, metal, ivory, etc.

In making holes in rock there are several ways; the oldest, by percussion only, accompanied by intermittent rotation of the drill bit. Very little rock is wrought by drills proper, having steady advance and continuous rotation, and breaking up all the material they remove. There is one type of rock drill which has continuous rotation and continuous advance, and which cuts out an annular channel, leaving in the centre a solid core which may, with care, be removed for inspection. The first and the third of these types—that is, the percussion drill and the ring or rock drill—are most generally in use. I shall in this paper treat of percussion drills only.

The drill proper is the drill bit, although usage sanctions the use of the word drill for the entire machine which guides or which drives the bit. Up to twenty years ago, there were very few drills driven except by hand power, the drill shank being either struck directly by hammer blows or raised by some machine and driven forward by a spring. We will speak of these two methods as the hand drill and the hand-drilling machine systems. So much more work can be done by hand-drilling machines than by hand drilling pure and simple, that the next step was to introduce a machine to imitate hand percussion drilling, with much greater force than by hand direct or by hand power machines. Of course the use of steam and of compressed air in a cylinder, to effect percussion by a drill bit fastened to the piston rod

of the cylinder, naturally suggests itself; the rotation of the drill bit on the back stroke and its advance lengthwise, to correspond with the depth of the cut taken at each stroke, being effected by many mechanical devices. Such a device constitutes the rock-perforating machine; or "rock drill" for short.

As ordinarily constructed, there is a double-acting steam engine, having a piston with a very long head, say 3 inches in diameter, the rod having the drill shank clamped to it direct. The valve motion of this machine, making as it does 300 strokes per minute and upwards, must be effective, simple, durable, compact and, if possible, economical of steam. Ordinarily the distributing valve is a slide valve, or its modification, the reciprocating piston valve; the cylinder having ports from each end of the steam chest in which the valve moves to and fro. The motion of the valve is effected by one of three systems; by lever, or tappets struck by the piston head, or by a boss or its equivalent upon the piston rod; by having the machine duplex; and by fluid transmission without any mechanical connection proper. Some machines have no separate external valve; the piston, or a sleeve thereon, performing the valve office.

The rotary motion is generally given by flutes upon the stem of the piston working in a fluted ratchet nut, although there are other methods; for instance, the stem may be in the back cylinder head and the ratchet nut in the piston head, etc. The forward feed of the drill bit may be by forward motion of the piston in the cylinder, the stroke getting longer each time; or by the stroke remaining the same in length, while changing its place in the cylinder bore; or the cylinder may be advanced. This feed may be either automatic, or by hand; and if automatic, it may be regular and independent of the hardness of the rock being penetrated; or it may be controlled by which this depth of feed may be regulated by hand while the actual work is done automatically; or the forward feed may be determined by the depth of the last cut taken, so that if the drill strikes a hard place and does not cut as deep as when it is working in a soft place, the feed will be lessened. Generally the cylinder heads are cushioned with rubber protected with metal discs, or else there is very great cushion of the driving air or steam, in order to prevent the heads being knocked out by the drill bit coming suddenly into a soft place or a cavity.

As regards the mounting of the machine, this depends upon the

work to be done. If in quarrying or in other surface work, where the holes are all vertical or nearly so, the most common mounting is a tripod having adjustable legs, which are weighted down to keep the machine in place, and are often extensible to allow of more perfect and steady placing. The steam or compressed air is led to the neighborhood of the drill by steam pipes of iron, and from them by flexible hose to the steam chest. If the machine is for gallery work, in which all the holes are horizontal or nearly so, the machine is generally mounted upon a column having thrust screws which jam it against the top and bottom of the heading and hold it firm. In some cases this column is threaded, to allow the machine to be adjusted in height, in others the column is smooth and the machine is clamped at the desired height. In others this column is mounted upon a carriage and has transverse motion by slides upon the carriage frame. In all cases where a column is used, the machine has free adjustment in the horizontal plane and also swings in a vertical plane, so that it has free adjustment after the column is in place, in height, in the vertical angle and in the horizontal angle.

For that class of surface work where it is desirable to channel, or cut a number of holes in a line close together, there is an arrangement permitting this to be done. In some cases, for tunnel work, several drills are mounted upon a carriage and all are given lengthwise advance at the same time by this carriage, although each one may cut at any desired angle in the vertical or horizontal plane. For those situations where it is necessary to cut down close to the floor, a special type of frame must be used, and in some cases a special type of machine. I show on the screen several types of drills and several styles of mounting them. Some of the illustrations are from working drawings of the makers.

It can be said about the tappet system of working a valve, that, while giving a positive motion to the valve, there is liability to and there always has been trouble from breakage of the tappets and their slight working parts, which are struck so many times a minute with such great force. This trouble is increased in frosty weather and this brings the repair bill up very high. Some opponents of tappets think that because the piston has to strike the tappet on the working stroke, the full force of the blow is not delivered upon the rock; but it seems to me that this is but a trivial matter.

The duplex system is not in use in this country. Because of the

very rough usage that the rock perforating machine has to undergo, it is well that there be as few exposed working portions as possible.

In reference to automatic feed; this is not generally applied to small machines, because these being used underground, light weight and simplicity is desirable. Where automatic feed is used, but one man is needed to work the machine.

In reference to steam or air cushion, those who oppose it do so on the ground that the motive fluid must be introduced by this means, in front of the advancing piston; so that, although the head is preserved from being knocked out, the blow is lessened in force and deepness.

Rotation is for two purposes; first to make a perfectly round hole, and second to make the bit work not only by mashing away the rock in front of it, but also by wedging off a portion of rock between the new cut and the last old one. To this a third reason may be added, that it preserves the edge of the bit, thereby lessening the wear and saving cost of sharpening.

The bits are of various shapes; the most common being in the form of an X. For loose, seamy rock, a Z shape is found to answer well.

The adjustability of the tripod may seem a very little thing, but it means a great deal. A machine properly mounted will work where two men cannot work with hammers upon the face of the cut. In a seam only two feet thick, like that at Port Henry, New York, the machine has to work nearly flat, with the legs spread apart.

By employing a lateral arm clamped to the column, the drill may be moved in or out on the arm, and the arm moved up or down or moved around, thus commanding a large portion of the breast without moving the column. The column should thrust against short pieces of timber at top and bottom. Columns over 8 feet long are apt to give give trouble from vibration. For a very large work, as tunnels, the column may be used to work out a drift of considerable size in extension of the line of the roof, and the tripods used to take up the bottom. This has the advantage of dividing the drilling ground and enabling more men to be used.

The disadvantage of the carriage is that the ground must be cleared before the carriage can be run up to the heading.

As regards the size of drills to use: 5-inch is used for submarine work, mounted upon a scow or frame; for deep heavy tunneling,

mounted upon a carriage; and for deep rock cutting, mounted upon a tripod. This size will drill from 1 to 40 feet deep and from 2 to 6 inches in diameter.

The 4-inch is for tunneling, heavy straight grading and quarry work, and where 12 to 20 feet holes, 2 to 4 inches in diameter, are to be made in very hard rock.

The $3\frac{1}{2}$-inch and the 3-inch are the most used; being found in quarries, railroad tunnels, grading, sewers and mining; the $3\frac{1}{2}$-inch drilling a 12 feet hole, $1\frac{1}{2}$ to $2\frac{1}{2}$ inches in diameter, and the 3-inch drilling an 8-feet hole, 1 to 2 inches in diameter. Below this size there are the $2\frac{3}{4}$ and $2\frac{1}{2}$-inch drills.

For horizontal drilling the capacities are about $\frac{1}{4}$ less. As regards the capacity of these drills, I annex some figures showing what they will do in various kinds of rock, and in some cases showing the rate of hand work in the same rock.

There is some work, such as cutting slate, granite and marble, where blasting cannot be used for fear of breaking the stone, and in this case 2-inch holes are drilled in a row, two inches apart, and the connection broken down by throwing out the rotation gear, and working out the stone between the holes with a drill bit having a flat point.

As far as possible, the drilling machine should be light, compact, strong, portable, quickly set up and moved, simple in construction, readily repaired at the shaft-head, economical of steam or compressed air, rapid in motion, free from trouble in freezing up where compressed air is used. If possible, the drill should be withdrawn automatically when desired. The machine should strike the blow variably according to the rock being entered. It is desirable that the hole be churned out by the machine itself, and that the bit shank may be quickly attached to the piston rod. Some like self-feed, some do not. The machine must be readily taken apart and kept clean and in working order. It must drill deep at one setting up. The heads should not knock out, and the tappets, if there be any, should not break. The piston rod should be large.

In the Johnson machine there is self-feed and steam or air pull-back. The whole cylinder may be slipped through the body clamp to or from the rock, thus adding to the length of the feed.

In the Bryer drill there are but two working parts—the piston and the rotation bar, the piston being its own valve, regulating the admis-

sion, cut-off and exhausts by annular grooves in the cylinder and piston. It is steam-cushioned at each end.

Place or works.	Diameter of piston.	Size of hole.	Material passed through.	Feet drilled per hour, day or month.	Cost per ft. (Drill hole.) Power.	Hand	Sav'g over hand drill'g
		in.					Per cent.
Yellow Jacket Silver Mining Co.	......	...	Conglom., Feldspar, Porph. and Quartz.	1160 ft. mo.	$1.02½	......	50
Overman Mining Co.	......	...	Hard blasting rock & Quartz.	2000 ft. mo.	.18¾	......	33⅓
Musconetcong Tunnel.	5 in.	...	....	25 to 40 ft. per day.	....	......	......
Sierra Nevada Mining Co.	......	...	Porphyry.	....	.32	......	50
Eagle Harbor and Ahnepee.	5 in.	...	Trap and Limestone.	5387 / 4491 working seasons '75, '76, '77.	....	......	......
Ausable Forks, N. Y.	......	1½	Feldsp'r, ore and Feldspathic rock.	....	....	......	50
Canada Pacific R. R., Manitoba.	5 in.	...	....	207 ft. 7 in. in 3 days.	....	......	......
Millstone Point, Conn.	......	2	....	7 ft. 8 in. per hr.	....	......	......
Iron Mountain Co., Mo.	......	3¼	Iron ore, Porph. and Limestone.	45 ft. per d. of 10 hours.	.23$\frac{3}{7}$	.83	66
Knoxville, Tenn.	......	...	Marble.	140 ft. P. D. 23 " " hand 3 men.	....	......	......
Chicago & Colorado Mining Co.	4 in.	...	....	22 in., 1 man 10 hours. 5 ft. in 30 min., P. Drill.	....	......	......
Paxton Furn'e, Pa.	......	2¼	Limestone.	45 ft. P. Dr. 8 " hand.	.12	.50	75
Leesport.	3¼ in	...	Limestone.	40 ft., P. D. 3 ft. 4 in. by hand, 1 man	....	......	......
Denver & South Park R. R.	3½ in	...	Granite, Iron rock.	80 ft. in 10 hours. 9 ft. with 2 hands.	....	......	......
Wakefield Marble Quarries.	3 in.	2	Marble.	65 ft. per day.	....	......	......
Diamond Hill Granite Co.	3 in.	...	Granite.	40 " " "	....	......	......
Steelton, Pa.	......	...	Limestone.	100 " " "	.04½	.61	......
Georgetown, Col.	......	...	....	90 " " "	....	......	......

In the Rand drill the tappets are moved by the piston instead of by projections upon the piston rod. The feed in the machine is by hand, a square-threaded screw carrying the cylinder along on its bed-plate, which latter has a vertical pivot.

In the Burleigh tappet drill there is a trough with ways on each side, in which the cylinder slides. The screw feed is automatic. The piston rod, which has a double annular cam and spiral grooves, controls the valve, effects the feed, and causes bit rotation. It is sometimes mounted in gangs in a frame or carriage, two on a horizontal bar across the top and two at the bottom, each having adjustability in these planes.

The single drill is mounted on a threaded telescopic iron column, having a single sharp point at the bottom and an iron thrust claw at the top. This column may also be used horizontally for shaft work.

The new Sergeant drill, made by the Ingersoll Rock Drill Co., was described at length, and illustrated, by Mr. F. L. Miller, in a paper before the Engineers' Club of Philadelphia, September, 1880. I show upon the screen the working drawings of the latest form. The principal peculiarities are in the valve and the ports and passages. The valve is a cylinder, having flanges somewhat smaller than the cylindrical steam chest, in which the valve slides upon a central bolt, which serves to hold the chest heads together. The chest heads have rubber cushions. The piston is of great length, and has a cavity as long as the piston stroke, so that it will always register with the ports and allow the steam exhaust from the valve to be exhausted therein. We will suppose the valve at one end of the stroke and steam let into the chest; the steam will pass to the cavity between the flanges, the steam being exhausted at the other end there will be no opposition to the valve moving towards that end, by reason of the steam rushing past the flange nearest the valve cylinder end. The cavity in the main piston prevents the valve being shifted until the main piston is nearly at the end of its stroke, when it will uncover the exhaust port of the valve cylinder in which the steam is confined, the steam passing into the upper port then to the opposite end of the chest, then by the lower port to the cavity in the piston. Rotation is by a fluted bar and nut; and feed by hand.

RADIO-DYNAMICS.

By Pliny Earle Chase, LL.D.

Abstract of lectures delivered before the Franklin Institute, March 10 and 17, 1881.

Your committee have invited me to lecture upon some of the results of investigations in which I have been specially engaged. My subject is given, in one part of the announcement, as astronomy; in another, as the music of the spheres. The former title is so far appropriate as it designates the source from which the greater part of my discoveries have been derived; the latter, as indicating the universal harmonies which are manifested, both by atoms and by stars, by microscopic and macrocosmic spheres alike, and which are, as I shall try to show you, the necessary results of the plan which has established the stability of the physical universe.

It will be impossible, in two lectures, to do more than glance at a few of the instances of prevailing rhythm, but I think you will find those which I have time to bring before you quite sufficient to serve as the solid groundwork of a science which is both the oldest and the newest of all sciences—the science of photo-dynamics or radio-dynamics. I call it the oldest, because we are told in Genesis that the first act of the Creator, in educing order out of chaos, was the command: "Let there be light;" the newest, because its right to recognition is as yet but sparingly and somewhat hesitatingly accepted, and because nearly all the materials, with which it has to deal in its systematic coördination, have been collected within the last quarter of a century.

The scientific spirit strives always to ascend from the special to the general; from multiplicity to unity. The Greek philosophers looked, in turns, to each of their four elements—earth, air, fire and water—as the basis of all things. Newton, in his "Principia," demonstrated many propositions which are applicable in all fields of physical investigation, but he used them only for explaining the motions of the various members of the solar system. He spoke, however, of an "æthereal spirit," as a possible medium in universal gravitation, but without giving any hint of believing that any of its properties were within the reach of physical research. Franklin's experiments in electricity furnished a foundation for electro-dynamics, and led to a

belief, which is still widely held, that in the various forms of electrical manifestation the clue to all physical activity is to be found. Mayer, Joule and their collaborators opened the gates of that fairyland of science which Tyndall has so admirably described in his "Heat as a Mode of Motion," and there are many who now believe that all material phenomena are susceptible of an explanation by thermo-dynamic laws.

The theory of the "correlation of forces," which teaches that light, heat, electricity, magnetism and chemical affinity are all forms of a single energy, and that they all may be interchangeably converted, provided the proper conditions are observed, may be thought to imply that neither of the correlated sciences is entitled to any precedence over the others, but that each of them becomes tributary to the general science of universal physics, so far as it develops laws which are of universal application.

Sir John Herschel appears to have been the first investigator who ever proposed any numerical estimate of the energy of light. It is a well-known proposition that the velocity of wave propagation, in elastic media, varies directly as the square root of the elasticity and inversely as the square root of the density. He accordingly stated, in his "Familiar Lectures on Scientific Subjects" (pp. 281-3), that the elastic force of the air, in its resistance to compression, would require to be increased "*in proportion to the inertia of its molecules*" more than 1,000,000,000,000-fold, to admit of the propagation of a wave with the velocity of light, and that this enormous physical force is perpetually exerted at every point through all the immensity of space. He also said (p. 218): "It must be remembered that it is LIGHT, *and the free communication of it from the remotest region of the universe,* which alone can give and does give us the assurance of a uniform and all-pervading energy."

In the eloquent extract which is quoted by Tyndall (*op. cit.*, 4th ed., section 707), Herschel had previously stated that "the sun's rays are the ultimate source of almost every motion which takes place on the surface of the earth." Tyndall, with equal eloquence (*Ibid.*, section 724), describes the flux of power which "rolls in music through the ages," and shows that all "the integrated energies of our world . . . are generated by a portion of the sun's energy which does not amount to $\frac{1}{2300000000}$ of the whole."

These extracts seem to furnish a sufficient reason for looking upon

solar radiation as the basis of all terrestrial physics, and upon radio-dynamics, or the science which refers all physical activity to centres of energy, as the universal physical science. Gravitation, cohesion and chemical affinity are directly concerned only with centripetal phases of force; inertia, in orbital and explosive motions, introduces a kind of centrifugal action; heat, properly speaking, seems to be wholly centrifugal, for the approach of particles when heat is radiated can hardly be attributed to thermo-dynamic action; electricity and magnetism, as positive and negative, boreal and austral, are both centripetal and centrifugal; light, according to the undulatory hypothesis, also represents both phases of activity, in the alternate contractions and expansions of wave propagation, as well as in the phenomena of radiation, refraction, reflection and coloration.

Electricity and light have been connected, and to some extent identified, by means of investigations which were begun by Weber and Kohlrausch, in Germany, and continued by Thomson, Maxwell, Ayrton and Perry, in England. As a result of those investigations, it has been found that electro-magnetism is related to electro-statics, somewhat as momentum to mass, the electro-magnetic unit being equivalent to the electro-static unit multiplied by the velocity of light.

Maxwell, accordingly, regarded light as an electro-magnetic phenomenon. It seems to me more logical to regard electro-magnetism as a luminous or radial phenomenon, for the following reasons:

1. Because the velocity of light is only one factor of electro-magnetism, but it is the important factor which constitutes it a force.

2. Because we have no evidence of electro-magnetic action in space, while we have much evidence of the action of light.

3. Because the eminent practical observers, who have studied the phenomena of terrestrial magnetism most carefully, have concluded that there is no specific magnetism in the sun and moon to influence the terrestrial magnetism through induction.

4. Because the mass-factor, which constitutes an important though subordinate element in all thermal, chemical, electrical and magnetic phenomena, is mainly, at least so far as it appears most obviously in those phenomena, a terrestrial factor.

5. Because it is better to designate the solar radiations by a name which will be universally recognized as appropriate, than by a name which has been generally applied only to local phenomena.

A still stronger and perhaps conclusive reason for regarding photo-dynamics as a special and principal department of radio-dynamics, is the fact that the velocity of light, as I propose to show you, is an important factor of gravitating, as well as of electro-magnetic action. In studying the phenomena of gravitation, there is no necessity for introducing any other elements than those of simple *vis viva*, mass and the square of the velocity. If the limit of efficient velocity can be shown to be the velocity of light in both departments, the law of parsimony would exclude the electro-static unit, unless it can be shown that it is a necessary element of mass. This has never yet been done. If the necessity should be demonstrated hereafter, it is more likely that it will be found to depend upon some modification of the fundamental velocity of light than upon any independent activity which can be regarded as purely electrical.

The chief postulate of photo-dynamics may be stated as follows: *All physical phenomena are due to an Omnipresent Power, acting in ways which may be represented by harmonic or cyclical undulations in an elastic medium.*

The Omnipresent Power is scientifically required by the law of harmony; the harmonic or cyclical undulations, by the law of permanence or stability; the representative elastic medium, by the law of equal and opposite action and reaction. All questions as to the reality or nature of the supposed medium are of minor importance. Although my investigations have strengthened my own belief in the reality of an all-pervading æther, we are only required to recognize the existence of phenomena which involve such actions, and can be explained by such laws, as have been deduced from the motions of the atmosphere and other elastic fluids.

The following well-known laws have an important bearing upon photo-dynamics:

1. Cyclical activities may often be accurately represented by formulas which introduce mean or average velocities and mean *vis viva*. This is the foundation of Maxwell's theory of the equality of mean *vis viva* in the molecular movements of different gases at equal temperatures, and of Pfaundler's discovery that in estimating the heat of dissociation, the mean should be taken between the temperatures of incipient and of complete dissociation.

2. The projectile force, which produces flight or cyclical motion against any central acceleration or retardation, is equivalent to the

mean acceleration or retardation multiplied by one-half the time of flight or cyclical motion.

3. The velocities of wave motion in elastic fluids, and of cosmical and molecular orbital motion, can all be expressed by a common formula.

4. Every periodic vibrating or orbital motion can be regarded as the sum of a certain number of pendulum vibrations.

5. Mean *vis viva* may be represented by the *vis viva* of centres of oscillation.

6. The distance of the centre of oscillation from the centre of relative stability is at two-thirds of the length of a linear pendulum, or at the square root of four-tenths of radius in a rotating sphere.

7. The acceleration of any force, which is uniformly diffused from or towards a given centre, varies inversely as the square of the distance from the centre.

8. Times of revolution, under the action of such forces, vary as the three halves power of the distance; distances vary as the two-thirds power of the time.

9. Centres of inertia, or nodes, in a vibrating elastic medium, tend to produce harmonic nodes.

10. The force of planetary projection should be referred to perihelion; the force of incipient subsidence, to aphelion.

11. The mutual inter-actions of cosmical, molecular or atomic bodies are proportioned to the respective masses; actions which are considered with reference to a single active centre vary directly as the mass and inversely as the square of the distance.

12. In elastic atmospheres the densities decrease in geometrical progression, as the height above the surface increases in arithmetical progression.

13. Living force, or *vis viva*, is proportional to the product of mass by the square of the velocity.

14. The distance of projection against uniform resistance is proportioned to the living force.

15. In synchronous orbits, the mean velocity of rectlinear oscillation is to the velocity of circular orbital oscillation as twice the diameter is to the circumference.

16. In a condensing nebula, the velocity of circular orbital revolution is acquired by subsidence, from a state of rest, through one-half of radius.

The following additional propositions may be readily deduced from the foregoing.

17. The acceleration or retardation of a centripetal force varies as the fourth power of the velocity of orbital revolution.

18. In cyclical motions, the resultant of all internal forces must be in equilibrium with the resultant of all external forces, at the expiration of each half cycle.

19. The modulus of cyclical motion is equal to the product of acceleration by the square of the time of a half cycle.

20. The sum of all external forces may, therefore, be represented (2) by a velocity which is equivalent to the mean or resultant internal force acting for one-half of the cyclical time.

21. At the extremity of a linear pendulum, the influence of a central force on the centre of oscillation is nine times as great as on the centre of suspension.

22. The limiting *vis viva* of wave propagation is five-ninths of the mean *vis viva* of the oscillating particles.

23. In condensing nebulæ, rupturing forces which are due to central subsidence may be represented by fractions in which the denominator is one greater than the numerator.

24. In synchronous rotation and revolution, the nucleal radius varies as the three-fourths power of the limiting atmospheric radius.

25. The variation in mean *vis viva* of gaseous volume is to the variation in *vis viva* of uniform velocity as 1 is to 1·4232.

26. The mean thermal and mechanical influences of the sun must be in equilibrium.

27. The collisions of particles, in subsiding towards a centre of force, tend to form belts at the centre of linear oscillation.

28. The limiting velocity between tendencies to aggregation and tendencies to dissociation is to the velocity in a circular orbit as the ratio of the circumference of a circle to its diameter is to the square root of two.

29. In explosive, as well as in cyclical motions, equilibrium must be established between internal and external forces.

30. Apsidal and mean planetary positions must also be controlled by like tendencies to equilibrium.

31. Undulations in an elastic medium maintain the primitive velocity which is due to their place of origination.

32. When two or more cyclical motions are combined, they must all be modified by the tendency to conservation of areas.

33. In expanding or condensing nebulæ, the conservation of areas maintains a constant value for the modulus of rotation.

34. Instantaneous action between different masses or particles by mere material intervention is impossible.

35. In synchronous motions about different centres, the mean distances from the centres of motion vary as the cube root of the masses or other controlling forces.

36. Constant velocities, in a homogeneous elastic medium, represent constant living forces.

In applying these general principles, we must expect to meet with perturbations, arising from the adjustment of opposing tendencies. If the problem of three bodies is so difficult, in astronomy, as to defy all efforts at satisfactory solution, the attempt to grapple with all the intricacies of elastic interaction may also defy the ordinary methods of mathematical analysis. And yet, by paying proper regard to mean values, it is possible, through very brief and simple processes, to get approximate determinations of important astronomical and physical constants, in which the error is less than in the ordinary approximations which require long, tedious and intricate calculations.

Lockyer's late spectroscopic researches have awakened a new interest in the old theory, that all chemical elements are merely different forms of condensed æther, and that the æther itself is only a universal atmosphere. Taking this theory as a provisional hypothesis, there can be little question that hydrogen is the element which resembles the æther most closely, and which may, therefore, be regarded either as the first step in elementary condensation, or as the transmitter of primordial undulation. It is the lightest of all known substances; it is hyper-elastic, being the only gas in which the elasticity increases faster than the condensation; it is always present in solar explosions, if the evidence of the spectroscope is trustworthy; the height to which it is thrown, and the rapidity of its diffusion, in these explosions, indicate a force and velocity which can be best explained by photo-dynamic influence; there are many reasons for believing that it is the outer envelope of the sun; and it presents many features of peculiar interest in connection with Lockyer's basic lines, which furnish simple harmonic indications of great significance.

In order to illustrate some of the properties of hydrogen I have

prepared a pipe and some some soap suds, for blowing bubbles, and by making a connection with the receivers I am able to inflate the bubbles with a mixture of oxygen and hydrogen.

You see with what rapidity the little balloons mount to the ceiling. I touch them with a candle and you are startled by their explosion. Doubtless most of you have seen the experiment before, and have learned that when the gaseous particles rush together, after the explosion, they are joined chemically so as to form water; but I think none of you have ever dreamed of any possible bond of union between the explosion and satellite revolution, or of weighing the bubbles in a scale with the sun. That there is such a bond, and that the sun can be thus weighed, I will try to show you.

Tyndall has told us (*op. cit.*, section 181) that the force of explosion, in one pound of hydrogen uniting with eight pounds of oxygen, is "equivalent in energy to the descent of a ton weight down a precipice 22,320 feet high"; it would, therefore, be sufficient to lift a ton to the top of such a precipice. If it were all concentrated upon the hydrogen alone, that gas would be driven entirely beyond the reach of the earth's attraction; but it carries with it the eight pounds of oxygen, and, notwithstanding this ninefold burden, if there were no resistance from the air, the watery vapor would be lifted more than two thousand miles before it would begin to fall to the earth again. The velocity with which it starts is more than five miles per second, or nearly one per cent. more than the velocity with which a satellite would revolve at the surface of the earth.

You have already learned that circular orbital velocity is acquired (16) by falling through half the distance to the centre; therefore the combining energy of water is more than sufficient, if it were not for the resistances of the air and of friction, to keep it in perpetual revolution. Those resistances do not destroy the motion; they merely change it into heat, electricity, magnetism, chemical affinity, molecular vibration, or some other form of cyclical oscillation.

Do you think that these harmonies are merely accidental, or that they can be so regarded with any reasonable probability? In order to remove any possible doubts upon the question, I will ask you to follow me still further.

According to the kinetic theory of gases, the particles are in perpetual motion, and the gaseous elasticity is owing to the force of

repeated collisions. You may accept or reject the theory as you please, but all the known properties of elastic fluids are such as they would be if the theory were true. It may, therefore, be safely assumed as a guide to new investigations. In subsidence from the satellite orbit of watery vapor, when the orbit velocity is increased twofold, the gravitating acceleration (17) is increased sixteenfold. Now, if we multiply this increased acceleration by the molecular velocity of hydrogen, the product is the same as if we multiply the original acceleration by the orbital velocity of the earth, so that the explosion of our soap bubbles furnishes us with all the data which are needed for weighing the sun and measuring its distance.

In order to make our approximations as close as possible, it is desirable to check, or confirm them, by finding some other harmony of a similar character. We look naturally, in the first place, to hydrogen's companion in its plunge down the mighty precipice, and we find that oxygen stands in a still closer relation to earth's velocity of rotation than that in which hydrogen stands to earth's orbital velocity. If we divide earth's equatorial circumference by the number of seconds in a sidereal day, we find that its rotating velocity is 1525·7 feet per second, which is precisely the velocity of oxygen, according to the experiments of Clausius, at the temperature of 4·8°C. This is within the limits of possible uncertainty of the temperature of water at its greatest density, the commonly accepted temperature being 4°C.

This harmony may be extended so as to include all gases through Maxwell's law of equality of gaseous *vis viva*.

Substituting atomic weight for gravitating acceleration, and remembering that orbital *vis viva*, in equal volumes, is proportioned to the gravitating acceleration, the mean velocity of hydrogen at 4·8°C. can be readily deduced from the mean velocity, at the same temperature, of any other gas of known atomic weight. If we adopt Regnault's value for the atomic weight of oxygen, 15·96, the mean distance of the sun is 92,769,000 miles; its mass 331,595; and the velocity of light is 186,400 miles per second. These results, though so simply deduced, are fully as trustworthy as any that astronomers have yet reached, after thousands of years of patient observation and tedious calculation.

MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES.

By ALEXANDER GRAHAM BELL.

A paper read before the Philosophical Society of Washington, D.C., June, 11, 1881.

In August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.* Shortly afterwards Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.† This explanation has recently been called in question by Mr. Preece,‡ who has expressed the opinion that although vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him, the ærial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm—every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory. He was thus forced—by the supposed insufficiency of the explanation—to seek in some other direction the cause of the phenomenon observed, and as a consequence he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science,‖ that audible sounds result from the expansion and contraction of the material exposed to the beam; and that a real to and fro vibration of the dia-

* Amer. Asso. for Advancement of Science, Aug. 27, 1880.

† Nature, vol. xxiii, p. 274.

‡ Roy. Soc., Mar. 10, 1881.

‖ April 21, 1881.

phragm occurs, capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone, shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the supports (*A B*) of the microphone might touch portions of the diaphragm which were practically at rest. It would of course be interesting to ascertain whether any such localization of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated (see Fig. 2).

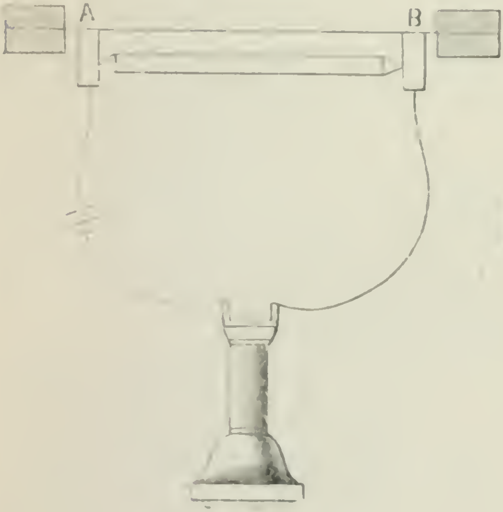


Fig. 1

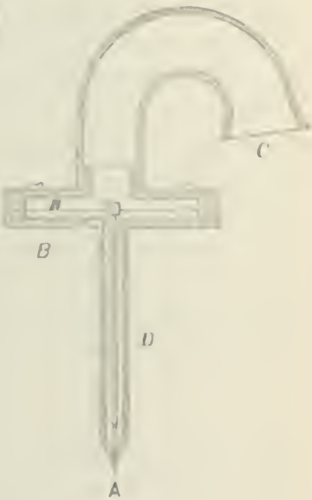


Fig. 2.

The instrument is a modification of the form of microphone devised in 1827, by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire (*A*), one end of which is rigidly attached to the centre of a metallic diaphragm (*B*). In Wheatstone's original arrangement the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body—like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are con-

veyed to the ear through a rubber hearing tube (*C*). The wire passes through the perforated handle (*D*), and is exposed only at the extremity. When the wire (*A*) was rested against the centre of a diaphragm upon which was focussed an intermittent beam of sunlight a clear musical tone was perceived by applying the ear to the hearing tube (*C*). The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm the sounds became weaker and weaker until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes microphone (see Fig. 1), no sound was observed. We were also unable to detect any audible effects when the point of the microphóne was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may therefore be reconciled with the positive results obtained in America by Mr. Tainter and myself. A still more curious demonstration of localization of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram), and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

In this experiment as in the case of the thin diaphragm absolute contact between the point of the microphone and the surface explored was necessary, in order to obtain audible effects. Now I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs, and that it is sufficient to account for the audible effects observed.

STORED-UP ELECTRICITY: FAURE'S SECONDARY BATTERY.

A few weeks since the scientific world in Paris was deeply interested by a paper read before the Société d'Encouragement de l'Industrie Nationale by M. Reynier, upon a new form of battery invented

by M. Camille Faure, who, following in the steps of M. Gaston Planté, had succeeded—according to M. Reynier in devising a battery in which forty times as much electric energy may be stored up as could be done by the Planté pile, the result being that a large amount of force easily convertible into mechanical work, or adaptable for electric lighting, could be stored up in small and easily portable cells, could be transported from place to place, delivered from house to house, and that the great problem of a domestic electric lighting and power supply was thus solved.

M. Faure's secondary battery is an application of a new discovery to the very beautiful and well-known secondary pile of M. Gaston Planté, which our readers will remember consists of two plates of sheet lead separated from one another and immersed in a glass jar of diluted sulphuric acid; if these two plates are connected for a time with the terminals of a source of electricity such a dynamo-electric machine or a voltaic battery, oxidation and deoxidation take place on the two plates respectively, and after the exciting battery has been removed, the lead cell continues to give off a polarization current of electricity as long as the deoxidation and oxidation of the lead plates continue by their returning to their normal condition. It was from the first observed that secondary piles of this construction produced better results after having been charged and discharged a great many times, a fact due no doubt partly to the increase of surface produced by the roughening of the lead plates under the decomposition, but chiefly to the formation of lead peroxide in increasing quantities, which was alternately deposited and decomposed as the cell was charged by the battery and discharged by the polarization current.

M. Faure (whose name is well known in the scientific world as the inventor of the battery which bears his name, and in which the carbon element in a Bunsen's cell is made in the form of a bottle which contains the nitric acid) has recently introduced an important improvement to the Planté cell, by which its capacity is largely increased, so that an apparatus constructed upon his principle is capable of producing a much greater current than that given off by a secondary battery of the old construction and of the same size. As the capacity of a secondary battery, other things being equal, is due to the thickness of the layer of lead peroxide formed upon one of the lead plates, M. Faure conceived the idea of coating each of the plates with a thickness of red lead maintained in its place by a sheet of felt attached to the plate by means of lead

rivets. Both plates having been similarly treated they are rolled together into a spiral, the felt performing the two-fold duty of separating the plates and holding on the coating of read lead. This couple is then immersed in acid contained in a cylindrical cell of lead and connected by its electrode to the poles of a dynamo-electric machine or voltaic battery, and after having been charged and discharged two or three times, the red lead coatings of the plates are found to have undergone a change, the one having been entirely transformed into peroxide of lead, while the other has been reduced to the metallic state, and as this result must be due to the oxidation of the red lead on the one plate and the deoxidation of that on the other, it would appear that what may be called the storage capacity of the apparatus depends upon the quantity of red lead carried by the plates.

There can be no doubt that in this way electricity may be "bottled up" and "stored" to an almost unlimited extent, and in this bottled-up condition can be carried in reservoirs to a distance, there to be utilized until it is exhausted, just as a reservoir of compressed air, or a coiled up spring, may be carried for any number of miles, and can be made to give out power whenever required at a distant station. All this is true enough and physically feasible, but the whole commercial success or failure of such a scheme must depend, as all commercial schemes must depend, upon its practical utility.

If electrical energy has to be conveyed from one place to another, it is a matter of small commercial importance in the abstract whether it is conveyed by means of metallic conductors or stored up in reservoirs and carried by road or rail; in this the commercial question involved being very much the same as that of the supplying of water by pipes or by water carts. There can be no doubt, however, about which system is, save in exceptional cases, the most convenient, and unless it can be shown that the charging and transmission of storage reservoirs offers advantages upon economical grounds, or very substantial conveniences of application over the system of transmission by conductors, we cannot see that its commercial application upon a large scale can be as remunerative to its proprietors as its promoters would wish to make the public believe.

That M. Reynier should have infused an undue amount of enthusiasm into his paper read before the Société d'Encouragement was natural, considering that he was describing for the first time results far in advance of anything of the same nature than had been achieved

before, and it was perhaps pardonable that he should, for the greater glory of the Faure battery have depreciated the capacity of that of Planté. But in justice to the latter gentleman, and also to arrive at a just appreciation of the real value of the new discovery, careful investigation and comparison between the two have to be instituted. This has been partially done by M. E. Hospitallier since M. Reynier read his paper, and the last number of our excellent conpemporary *L'Electricien* the results obtained are referred to, and the details of the experiments made are to be published in a succeeding number of the same periodical.

Criticising M. Reynier's paper, M. Hospitallier challenges several statements made in that communication. M. Reynier maintained that the Faure battery would give out 80 per cent. of the total power used in charging it. But as M. Hospitallier points out, under the best conditions not more than 90 per cent. of actual work can be transformed from mechanical into electrical energy by a dynamo-electric machine. M. Planté clearly demonstrated that his secondary battery could only give out 88 or 89 per cent. of the power charged into it, and as the difference between it and the Faure battery is one of degree and not of principle, it is not probable that a greater percentage than this could be obtained; possibly it would be less. Finally, a loss of 20 per cent. at least must be allowed for in converting the electrical power in the battery into mechanical force. Making allowances for all these losses it follows that the utmost useful work that can be got from the battery is 52.5 per cent. of the energy employed in charging it, while at the present time it is easy with the ordinary system of conductors to obtain 60 per cent. Passing on to the question of the power which can be stored up in the Faure battery, M. Hospitallier makes an important statement in reply to the assertion of M. Reynier, that this battery can store up forty times as much force as the Planté battery. In conjunction with M. Frank Géraldy, M. Hospitallier has conducted a series of experiments on the Planté battery. The details of these experiments will be published shortly, but the results are given as follows in *L'Electricien*: "Admitting on the one hand as correct the figures given by M. Reynier, that is to say, that a Faure battery, weighing 165 lbs., can give out one horse power during one hour, and on the other hand our experiments on the Planté batteries, the storage power of the Faure batteries varies from one and a half to three times the power of the Planté, according to conditions which we

shall shortly publish. This result is very far from the forty-fold result given by M. Reynier, who obtained it no doubt from imperfect or badly-proportioned Planté batteries, and these results cannot consequently be accepted."—*Engineering.*

Reynier's Constant Battery.—M. Reynier has made a modification of Becquerel's hydro-electric battery, which is comparable to the nitric acid couples in energy without having their inconveniences. The zinc is plunged into a solution of caustic soda; the negative electrode, which is of copper, is depolarized by a solution of sulphate of copper, separated from the alkaline liquid by a permeable cell. The couple thus constituted is constant; its electro-motive force varies from 1·3 to 1·5 volts, according to the concentration of the liquids. The moderate conductibility of the liquids is modified by the addition of salts suitably chosen, and the resistance of the porous cell is reduced by making it of parchment paper, as had previously been done by F. Carré. This battery can be regenerated so readily that the inventor hopes to make it applicable with great economy to small electric motors and to lighting private apartments.—*Comptes Rendus.* C.

Reversion of Photographic Images by Prolonged Illumination.—Janssen has found that photographic images may be inverted and pass from the negative to the positive state by the prolonged action of the light, which produces them. At Meudon the solar images are obtained in a time which varies, according to the state of the atmosphere and the nature of the phenomena, which are to be studied; the time is rarely greater than $\frac{1}{1000}$ of a second when they wish to obtain the photospheric granulations. When the photographic plates are prepared with gelatino-bromide of silver the time may be reduced to less than $\frac{1}{20000}$ of a second. If one of these dry plates receives the light for half a second or a second, the development brings out a positive image, with a white disc and black spots, as the sun appears through a telescope. This positive image may be as well defined as the negative image, which it has replaced. There is an intermediate time between those which give the opposite images, for which time the image is neither positive nor negative, but the plate presents a tint sensibly uniform. Similar inversions may be produced in views of landscapes by giving a sufficiently long exposure.—*Compt. Rend.* C.

Thermo-Dynamics of Liquid Surfaces.—Van der Mensbrugghe is still continuing his study of the application of the second law of thermo-dynamics to the variations of potential energy in liquid surfaces. He is giving special study, by means of some of the most salient facts, to the great cycle of change which embraces the evaporation of the superficial layers of ocean waters, the elevation of vapors into the atmosphere, their condensation into mists and clouds, their fall in rain or snow, the consequent production of glaciers, torrents and rivers, the circulation of water courses, and their return to the bosom of the ocean. He hopes to attract the attention of meteorologists to his investigations in order to induce new and extensive researches in the direction of his inquiries.—*Bull. de l'Acad. Belg.* C.

Intermittent Luminous Signals. In the ordinary use of lamps for light-house signals the intermittences are produced by a diaphragm which moves before the light, so that the fuel is wasted during the eclipses. At present the average waste of light is about sixty-five per cent., but if a signal was sent twice a minute, sufficient to indicate the first two letters of the light-house, there would be a waste of about ninety per cent. In order to remedy this extravagance Mercadier proposes to adopt a Dubosq lamp with a round wick and a tube in the centre of very small diameter, through which a jet of oxygen can be discharged upon the top of the wick. In spite of the high temperature of combustion, the lamp does not heat much; it consumes little petroleum, and the wick does not crust. Therefore it will operate for many days without being trimmed or filled anew. The intense flame is produced by the combustion of petroleum vapor at the centre of the jet, and the surrounding film of air being a bad conductor the lamp heats only at the top of the burner. The oxygen is enclosed in a reservoir, under suitable pressure, which in his apparatus does not exceed four millimetres (·157 in.) of mercury; it first passes through a manipulator, which has a form similar to that of the key of a Morse instrument, traversing a caoutchouc tube, which is pressed together when the key is at rest. Upon depressing the key the pressure upon the tube ceases, and the oxygen reaches the flame; when the key is released the oxygen jet is stopped. In this manner the flow of oxygen is manipulated as simply as the electric current in the Morse system. The rapidity of manipulation is more than sufficient for all the requirements of optical telegraphy. A method somewhat similar has been contrived by Mercadier for the electric light.—*Compt. Rend.* C.

Remarkable Solar Protuberance.—About 11 A.M. on the 30th of August L. Thollon noticed a small and very brilliant luminous jet near the sun's equator. About 12 h. 45 m. it had attained prodigious proportions, while still preserving the form of a luminous jet from a direction nearly normal to the border of the sun. The result of frequent measurements indicated a height of protuberance at least equal to half of the sun's radius, or more than 200,000 miles. Whilst the lower and middle parts of the protuberance gave a deviation of the C line towards the violet end of the spectrum the summit presented a similar deviation towards the red end.—*Compt. Rend.* C.

Temperature of Least Resistance in Steel.—It is well known that a steel that is very flexible when cold breaks at the blue annealing temperature. It has generally been considered that the purer the iron is the less subject it becomes to this defect, but the workmen of the Ural mountains, who use irons of remarkable purity, have often observed the same fact. Mr. Adamson has found that the metal becomes powdery at a temperature between 260° and 370°C. (500 and 698°F.) or the temperature at which willow twigs take fire. This phenomenon seems to explain a large number of accidents, as for example the breaking of tires under the action of brakes and the fracture of riveted moulds and of machine arbors which become heated by friction.—*Ann. du Gen. Civ.* C.

Behavior of Gas under High Pressures.—The following are some of the results of Amagat's investigations upon the dilatation and compressibility of gases under extreme pressures. 1. The coefficient of dilatation, for temperatures not greatly exceeding the critical temperature, increases with the pressure until it reaches a maximum and then decreases indefinitely. 2. This maximum corresponds to the pressure at which the product $p\ v$ is a minimum. 3. The maximum diminishes with the increase of temperature and finally disappears. 4. At a temperature sufficiently high the compressibility is represented by the formula $p\ (v\ a) =$ a constant; a being the smallest volume that the fluid can occupy; for each gas a has a special value. The values of a for three important gases, at the freezing point and under normal pressure, are as follows: Carbonic acid ·00170, ethylene ·00232, hydrogen ·00078.—*Compt. Rend.* C.

Franklin Institute.

HALL OF THE INSTITUTE, June 15th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 113 members and 59 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that at the last meeting 16 persons were elected members of the Institute; also, that upon the recommendation of the Committee on Instruction the Board had decided to create three professorships, viz.: on mechanics, physics, and chemistry; also, that a chemical section had been authorized on the petition of the requisite number of members of the Institute.

On motion of Mr. Washington Jones the regular order of business was suspended, and the Institute proceeded to take a vote upon the repeal of Section 4 of Article III of the By-Laws, and the alteration of Section 1 of Article I, as published in the JOURNAL last month. Both were unanimously adopted, and the confirmation of the latter submitted to a vote of the stockholders present, with a similar result.

The committee on the subject of a reorganization of the Committee on Science and the Arts reported progress.

The following donations to the Library have been received:

Annual Report of the Chief Engineer of the Water Department for 1880. From the Chief Engineer.

Report of the Commissioner of Internal Revenue for year ending June, 1879. From the Commissioner.

Thirtieth Annual Report of the Indiana State Board of Agriculture, 1880. From the Board.

Reports of the Auditor-General on the Finances of the Commonwealth of Pennsylvania for 1878–80.
From Auditor-General, Harrisburg.

Annual Reports of the Secretary of Internal Affairs. Parts 1 and 2, 1879 and 1880. From the Secretary, Harrisburg.

Annual Report of the Secretary of Internal Affairs. Part 1, 1879–80. From the Secretary, Harrisburg.

Annual Report of the Chief Engineer U. S. A. for 1880.
From the Chief of Engineers.

Documents relating to the Colonial History of the State of New Jersey. By W. A. Whitehead. Vol. 1. 1631–87.

From the Author.

Transactions of the Department of Agriculture of Illinois. Vols. 9 and 11. 1871 and 1873. From E. Hiltebrand.

Wood-working Tools; How to Use Them. Boston, 1881.

From the Smithsonian Institute, Washington, D. C.

Transactions of the Society of Engineers for 1880.

From the Society, London.

Specifications and Drawings of Patents for September, 1880.

From the United States Patent Office.

Account of the Operations of the Great Trigonometrical Survey of India. Vol. 6.

From His Excellency the Viceroy and Governor-General of India.

Transactions and Proc. of New Zealand Institute. Vols. 1, 5 to 10, and 13.

Index to Vols. 1 to 8.

Catalogue of Tertiary Mollusca and Echinodermata of New Zealand. Wellington, 1873.

Geological Report on the Waikato District.

Hand Book of New Zealand. Wellington, 1880.

Appendix to Official Catalogue International Exhibition, Sidney. Wellington, 1880.

Palæontology of New Zealand. Part 4. Wellington, 1880.

Fifteenth Annual Report on the Colonial Museum, etc., 1879–80.

Catalogue of Exhibits International Exhibition. Melbourne, 1880.

Reports of Geological Explorations during 1877–79.

Catalogue of Stalk and Sessile-eyed Crustacea of New Zealand.

Catalogue of the Echinodermata of New Zealand.

From the New Zealand Institute, Wellington, N. Z.

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(1) Colera Asiatica memoire del Dott Filippo Pacini (2) Origini Della Lingua Poetica Italiana Del Dott C. N. Caix. From the Institute.

Meteorological Observations recorded at Six Stations in India in 1879.

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Anales del Instituto y Observatorio de Marina de San Fernando. By Don Cecilio Pujazon. Section 2. 1877 and 1878.
From the Institute.

Dr. Robert Grimshaw read a paper on "Percussion Rock Drills," illustrated by photographs of machines projected upon the screen. The paper is printed in this number of the JOURNAL.

Mr. Lorin Blodgett read a paper on "Textile Fibres Under the Microscope," illustrated by samples of them, and photographs of the same thrown upon the screen; also images of the silkworms at work. The cocoons, etc., were furnished by the Women's Silk Culture Association. Mr. Blodgett, after briefly mentioning the importance of the subject upon which he had undertaken to speak, said that he did not purpose to repeat the admirable general directions to silk-growers given by the Women's Association, but simply to show what the silk fibre

is, and how to handle it, so as to make it as valuable as the silks of Italy or France.

The fibre of silk as produced in the cocoon is a single, continuous and perfect fibre—the most perfect and durable of fibres, if properly treated; and it must be treated as a single fibre throughout. *Raw silk* is a definite number, five to eight fibres, as reeled from the cocoons, adhering in a body by the gum of the cocoon remaining on them—apparently a single fibre, but really a bundle of eight, as shown in the figure, representing a thread of Chinese *Tsatlees*, the best of the Chinese raw silks.

All silk fibres must be reeled in bundles of not less than five nor more than eight; if reeled in single, double or even triple fibres, from as many cocoons, they cannot be used as regular silk.

Floss silk is of two kinds; first, the light outer fibres of the cocoon, with the broken and imperfect fibres cleaned of gum, usually; but the better *floss silk* is reeled silk of not less than five cocoon fibres, not usually cleaned of gum, and not much twisted. This is more frequently called *singles*.

Tram is a combination of three threads, 15 to 24 original fibres, with more twist, 2½ to 3 turns to the inch.

Organzine is made up of two threads, twisted 12 turns per inch to the left, then doubled with 8 turns per inch to the right. This is the standard quality of thread for the best silk goods.

All who reel silk should weigh 500 yards of the raw thread, made up of five single cocoon fibres, making 2500 yards of single fibre. If the hank so reeled is above or below the standard weight, a greater or less number of cocoons should be united in the one raw thread.

These definitions are given because they are absolutely essential to success on the part of the American grower. He cannot make anything but raw silk in the gum, as reeled in the manner described; but he must know what the manufacturer who makes floss, tram and organzine demands, or his silk will be worth but one or two dollars per pound, when it should be worth six dollars per pound.

Spun silk is made from pierced cocoons, cocoon waste, and the waste mills using raw silk—that is, in its best form. It is carded and drawn with as much care as worsted wool, and forms a valuable element of many fabrics, particularly upholstery goods, trimmings and ornamental articles. All growers will, of course, have a part of their

product in these waste forms, but they alone do not make silk growing profitable.

The lecturer also described briefly other textile fibres, such as wool, cotton, jute, flax, ramie, etc., and showed on the screen magnified images of dyed silk with particles of iron appearing like knots on the fibre. These add weight to the goods, but reduce its wearing qualities and value. They are used to a great extent in some silks, the pound of silk sent to the dyer coming back weighing from 17 to 18 ounces, the additional weight being due to iron, tin or chemicals used in dyeing.

Mr. Orr said that he thought that flax had been ascertained to have a definite fibre of known diameter. It was an old homely fibre, but very good, and he was glad to say that the State of New Jersey had offered a premium of $5.00 per ton for flax straw grown in that State, in order to encourage that important industry.

The Secretary's report included the Servoss Gas Regulator, which was shown and explained. It contains no diaphragms of rubber or leather, liable to become clogged and stiff, but is made entirely of brass. It is attached to the discharge side of the meter, the house pipes being then screwed to the regulator instead of to the meter. The pressure of gas in the street mains is greater, in parts of a district at least, than is required to furnish full light without waste, this being made necessary by the length of the mains and the variable number of jets in use at different times. The regulator is intended to reduce the pressure in the pipes to which it is attached, so that no more gas shall pass than can be economically burned. In the Servoss Regulator there are two brass valves, one of which can be fixed and locked to limit the passage of gas for a fixed number of lights, while the other moves automatically and accommodates itself to the number of burners in use, the weight of this valve being gauged to the average gas pressure in the place at which it is to be employed. It has been in use for five years, and the patentee claims that it will save 15 per cent. of gas in this city without decreasing the illumination.

Amesbury's band saw filing machine was exhibited. It is designed not only to expedite the work of sharpening band saws, but to secure the even cutting of the points. It is said that an ordinary band saw contains from 500 to 1800 teeth, and that it takes an expert filer from 30 to 90 minutes to sharpen one, while a boy, who simply turns a crank, can with the machine do the same work better in from 10 to 15 minutes. Two special files are used, one for sharpening the face of

the teeth and gumming out the throat, and the other for sharpening the back of the teeth. The machine is adjustable for any size of teeth, and by the use of springs giving a variable pressure to high and low teeth irregularities are reduced, and the teeth brought to a level. The machines may be run by power or by hand.

G. W. Amesbury & Co. also exhibited improved solid bit expansion matcher heads, for tongueing and grooving, the heads being adjustable so as to give any thickness of tongue or groove desired.

A model of a section of Woodruff's new sleeping car was shown. The peculiarity about it is an arrangement of mattresses and supports, which does away with all boxes, leaving the upper part of the car entirely open in the daytime. The car, when used as an ordinary day car, does not show that it can be arranged as a sleeper, and when the berths are opened the upper and lower ones are substantially alike. The supports for the mattresses are made of wire, and can be compressed into one-half the space they occupy when extended.

Pole's differential car starter is different from all other devices, of which there are a great number. This starter works on the principle of changing the centre of gravity of the weight of the body to be moved or started, and consists in placing under a car or other vehicle a system of smaller wheels, to which the car body is attached, bearing upon the inner side of the rim of the wheels, which rest upon the track. The applied power is made to draw a suitable bar, which pulls on the axle of the larger wheels, and thereby throws the centre line of the large and small wheel out from the vehicle, and by the law of gravitation the small wheel, on which rests the vehicle, runs down the incline, and every time the horses pull they have the advantage of the differential leverage.

Thomas' adjustable table weighs but 15 pounds, and by a simple jointed arrangement, like lazy tongs, can be quickly adjusted at different heights, and maintained there; braces attached to a horizontal slotted bar with thumb screws, giving the table the necessary steadiness.

The Secretary read a letter from Gen. Hazen, in charge of the Signal Office at Washington, inviting college graduates to the opening which the Signal Bureau presents to young men of decided talent and scientific tastes.

There being no further business, on motion the Institute adjourned until the September meeting.

ISAAC NORRIS, M.D., *Secretary.*

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AN ACCOUNT OF EXPERIMENTS MADE BY A BOARD OF UNITED STATES NAVAL ENGINEERS WITH SCREW PROPELLERS OF DIFFERENT MATERIAL AND DIMENSIONS, APPLIED TO THE UNITED STATES FISH COMMISSION'S STEAMER "LOOKOUT," WITH THE HULL COPPERED AND NOT COPPERED.

By Chief-Engineer ISHERWOOD, U. S. Navy.

(Continued from page 49.)

In the following Table No. 1 will be found the data of the experiments. The quantities therein given are so fully described as to need no further explanation.

The friction of the engine, *per se*, that is to say, the pressure required to work the unloaded engine, is taken at 2 pounds per square inch of the pistons of the two cylinders. The remainders of the indicated pressures, after deduction of these two pounds, are the net pressures upon the pistons.

In order to obtain a single expression for the indicated and for the net pressures, they have been reduced to what they would have been had the engine consisted of the large cylinder alone.

Explanation of Table No. 2.

In Table No. 2 will be found the distribution of the power during each of the experiments whose data are recorded in Table No. 1, the capital letters at the heads of the columns, in both tables, indicating experiments with the screws designated by the same letters.

This distribution is necessary in order to understand what portions of the indicated horses-power developed by the engine are expended in the different operations connected with the propulsion of a vessel by a screw.

Of the indicated horses-power a portion is consumed in working the engine, *per se*, that is to say, the unloaded engine; and this portion must first be deducted, because, until the friction of the engine is overcome, no power can be applied to the screw, or externally of the engine; for it is obvious, that until the friction of the engine itself is counterbalanced, the piston cannot move.

After the deduction of this power, the remainder of the horses-power, called the net horses-power, is applied to the crankpin and does external work. A friction attends the net power, additional to that of the unloaded engine and proportional to the net power, let the latter be what it may; and the power required to overcome this friction is the power absorbed by the friction of the load, the articulations of the engine moving under the net pressure producing greater friction on them than is due to the mere weight of the moving parts and to the pressure of the packings.

Then, there are the horses-power expended in overcoming the skin or surface resistance of the screw blades experienced from the water in which they move. This power can be calculated independently when the data are known.

There still remain to be determined the portions of the net horses-power expended in the slip of the screw and in the propulsion of the vessel. These are ascertained as follows: The sum of the powers expended in overcoming the friction of the load and in overcoming the resistance of the water to the surface of the screw blades being deducted from the net horses-power, the remainder is divided between the power expended in the slip of the screw and in the propulsion of the vessel in the ratio of the speeds of the two; the pressure exercised by the screw forward in propelling the vessel, and backward upon the receding mass of water, constituting the slip of the screw, being the same. Hence, if the aforesaid remainder of power be mul-

ERS WITH THE DIFFERENT SCREW PROPELLERS APPLIED TO THE UNITED
OF SCREW FOR THAT VESSEL.

Hull coppered.

D	*E*	*F*	*G*
April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
6·	6·	4·	4·
153·4601	140·0500	143·5402	143·4125
10·4190	10·1929	9·9390	9·7493
20·1542	17·9739	21·9620	12·7150
45·7104	44·7881	39·6325	36·5380
43·7104	42·7881	37·6325	34·5380
9·5172	10·7662	9·2050	8·2954
7·5172	8·7662	7·2050	6·2954
25·8904	26·8090	23·4011	21·3831
23·1740	24·0926	20·6847	18·6667
63·6080	56.8784	51·5854	47·5153
36·9732	38·1705	33·4487	30·1167
100·5812	95·0489	85·0341	77·6320
60·8249	54·3385	48·9822	44·9144
29·2034	31·0797	26·1812	22·8556
90·0283	85·4182	75·1634	67·7700

SCREW EXPERIMENTS.

By Chief Engineer ISHERWOOD, U. S. Navy.

TABLE No. 1, Containing the Data of the Experiments made in 1880, in the Potomac River, by a Board of United States Naval Engineers with the Different Screw Propellers applied to the United States Fish Commission's Steamer "Lookout," for the Purpose of Ascertaining the Best Proportions of Screw for that Vessel.

		Hull not coppered.					Hull coppered.	
		A	*B*	*C*	*D*	*E*	*F*	*G*
	Date of experiments (a pair of runs on each day),	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.	April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
	Number of runs over the base of 11⅞ geographical miles,	6·	6·	6·	6·	6·	4·	4·
SPEED.	Number of double strokes made by the engine's pistons, and of revolutions made by the screw, per minute,	152·3085	158·1988	158·8560	153·4601	140·0500	143·5402	143·4125
	Speed of the vessel per hour, in geographical miles of 6086 feet,	9·5346	10·0974	9·6264	10·4190	10·1929	9·9390	9·7493
	Slip of the screw, in per centum of its speed,	24·4072	13·6772	18·0439	20·1542	17·9739	21·9620	12·7150
CYLINDER PRESSURES.	Indicated pressure on the piston of the small cylinder, in pounds per square inch,	37·9960	39·9330	35·906	45·7104	44·7881	39·6325	36·5380
	Net pressure on the piston of the small cylinder, in pounds per square inch,	35·9960	37·9330	33·906	43·7104	42·7881	37·6325	34·5380
	Indicated pressure on the piston of the large cylinder, in pounds per square inch,	8·3004	8·7000	7·618	9·5172	10·7662	9·2050	8·2954
	Net pressure on the piston of the large cylinder, in net pounds per square inch,	6·3004	6·7000	5·618	7·5172	8·7662	7·2050	6·2954
	Indicated pressure that would have been upon the piston of the large cylinder had the indicated pressure upon the piston of the small cylinder been reduced in the ratio of the net area of the piston of the large cylinder to the net area of the piston of the small cylinder, and the quantity thus obtained added to the indicated pressure upon the piston of the large cylinder,	21·9103	23·0038	20·4793	25·8904	26·8090	23·4011	21·3831
	Net pressure that would have been upon the piston of the large cylinder had the net pressure upon the piston of the small cylinder been reduced in the ratio of the net area of the piston of the large cylinder to the net area of the piston of the small cylinder, and the quantity thus obtained added to the net pressure upon the piston of the large cylinder,	19·1939	20·2874	17·7629	23·1740	24·0926	20·6847	18·6667
HORSES-POWER.	Indicated horses-power developed in the small cylinder,	52·4763	57·6466	51·7217	63·6080	56.8784	51·5854	47·5153
	Indicated horses-power developed in the large cylinder,	32·0041	35·0624	30 6356	36·9732	38·1705	33·4487	30·1167
	Aggregate indicated horses-power developed by the engine,	84·4804	92·7090	82·3573	100·5812	95·0489	85·0341	77·6320
	Net horses-power developed in the small cylinder,	49·7141	54·7594	48·8407	60·8249	54·3385	48·9822	44·9144
	Net horses-power developed in the large cylinder,	24·2926	27·0021	22·5927	29·2034	31·0797	26·1812	22·8556
	Aggregate net horses-power developed by the engine,	74·0067	81·7615	71·4334	90·0283	85·4182	75·1634	67·7700

tiplied by the speed of the slip expressed in fractions of the axial speed of the screw, the product will be the power expended int he slip, which being subtracted from the above remainder leaves the residue as the power expended in the propulsion of the vessel.

On these general principles, the corresponding quantities in Table No. 2 have been calculated. For facility of reference, the quantities have been grouped and the lines containing them numbered.

Line 1 contains the dates of the experiments. Line 2 gives the speeds of the vessel per hour, in geographical miles of 6086 feet. Line 3 gives the slips of the screws in per centum of their speed, calculated from their pitches into the number of their revolutions made per hour, and from the speeds of the vessel on line 2. On line 4 are the thrusts of the screws in pounds during each experiment; these quantities are what would have been given by a dynamometer had one been applied directly to the screw shaft and fulcrumed on the vessel. They are calculated from the quantities on line 19, by multiplying the latter by 33,000 and dividing the product by the speed of the vessel in feet per minute. Line 5 contains the number of double strokes made per minute by the pistons of the engine, and of revolutions made per minute by the screw. The quantities on lines 1, 2, 3 and 5 are taken from Table No. 1 and added for the sake of completeness to Table No. 2.

Distribution of the Indicated Horses-Power.

Lines 13 to 19, both inclusive, show the distribution of the indicated horses-power among the operations contingent on the propulsion of the vessel by the experimental screws. Line 13 contains the indicated horses-power, the quantities being taken from Table No. 1: and line 15 contains the net horses-power applied to the crankpin, also taken from Table No. 1. Line 14 contains the horses-power expended in working the engine, *per se*, or unloaded, the quantities being the difference between those on lines 13 and 15; they can, however, be obtained independently by calculation, the data being the speed of the piston per minute in feet obtained from line 5, the area in square inches of the piston of the large cylinder alone, and the pressure on line 7. Line 16 contains the horses-power absorbed by the friction of the load; these quantities are obtained by multiplying those on line 15 by 0·075, that fraction being the coefficient of the friction of the load. Line 17 gives the horses-power expended in overcoming the

resistance of the water to the surface of the screw blades. These quantities have been calculated on the assumption that a square foot of the helicoidal surface of the screws, moving in its helical path with a speed of 10 feet per second, has a resistance of 0·45 pound, which resistance increases or decreases in the ratio of the square of the helical speeds. Line 18 contains the horses-power expended in the slips of the screws, these quantities are calculated by multiplying the remainders of the quantities on line 15 after subtraction of the sum of the quantities on lines 16 and 17, by the quantities on line 3 expressed in fractions of the axial speeds of the screws. Line 19 gives the horses-power expended in the propulsion of the vessel, which power is all that is utilized of the entire power developed by the engine. The quantities are the remainders of those on line 15 after subtraction of the sum of the quantities on lines 16, 17 and 18.

Distribution of the Indicated Pressure on the Piston.

Lines 6 to 12, both inclusive, show the distribution of the indicated pressure. A single expression for this pressure being necessary, and the engine having two compounded cylinders—a small one and a large one—the desired single expression was obtained by reducing the experimental indicated pressure per square inch on the piston of the small cylinder, in the ratio of the areas of the pistons of both cylinders, and adding the quantity thus obtained to the experimental indicated pressure per square inch on the piston of the large cylinder. These are the quantities on line 6, and are taken from Table No. 1. Line 7 contains the pressure per square inch of the large piston alone, required to work the engine, *per se*. With both cylinders in use, this pressure was taken at 2 pounds per square inch of the pistons, and the equivalent, if applied to the piston of the large cylinder alone, was obtained by dividing 2 by the ratio of the areas of the two pistons and adding the quotient to 2, which produced the constant 2·7164 pounds per square inch, for the piston of the large cylinder alone, on line 7. Line 8 gives the net pressures applied to the crank-pin, in pounds per square inch of the piston of the large cylinder alone; these quantities are the remainders of those on line 6 after subtraction of those on line 7. Line 9 contains the pressures, per square inch of the piston of the large cylinder alone, absorbed by the friction of the loads; these quantities are the products of those on line 8 by the fraction 0·075 which is the coefficient for the friction.

Line 10 contains the pressures per square inch of the piston of the large cylinder alone expended in overcoming the resistance of the water to the surface of the screw blades. These quantities are the same proportion of those on line 8, that the quantities on line 17 are of those on line 15. Line 11 contains the pressures per square inch of the piston of the large cylinder alone expended in the slip of the screw. These quantities are the same proportion of those on line 8, that the quantities on line 18 are of those on line 15. Line 12 contains the pressures per square inch of the piston of the large cylinder alone, expended in the propulsion of the vessel. These quantities are the same proportion of those on line 8, that the quantities on line 19 are of those on line 15. Or, the quantities on line 12 are what remain after subtraction from those on line 8 of the sum of the quantities on lines 9, 10, 11 and 12.

The quantities on lines 20, 21, 22 and 23 are the per centum, respectively, which the quantities on lines 16, 17, 18 and 19 are of those on line 15. The quantities on lines 20, 21, 22 and 23 are also the per centum, respectively, which the quantities on lines 9, 10, 11 and 12 are of those on line 8. The quantities on lines 20, 21, 22 and 23 show, centesimally, the distribution of the net pressure and of the net power applied to the crankpin.

The quantities on line 23 express the relative utilizations of the experimental screws; that is to say, they express the per centum of the net power applied to the crankpin which the screws apply to the propulsion of the vessel. These utilizations are correct if the distribution of the net power be correct, but not otherwise. It is, therefore, of importance that the relative utilizations of the screws be obtained directly from data devoid of doubt. The quantities on line 24 express such results, and are obtained by dividing the cubes of the speeds of the vessel on line 2 by the net horses-power applied to the crankpin—line 15. The cube of the speed of the vessel expresses the useful work done, and the net horses-power expresses the power expended in doing it; the latter divided into the former gives the work done *pro rata* to power expended, therefore the larger the quotients of the divisions, that is, the larger the quantities on line 24, the greater will be the utilizations of the screws.

The quantities on line 24 afford the means of demonstrating whether the distribution of the power is correct; if it be, then the quantities on line 23 will have the same ratio to each other that

the quantities on line 24 have to each other. To find if this equality exists, the quantities on line 24 were divided into those on line 23, and the quotients are given on line 25. If these quotients were equal, then the distribution of the power would be exact; if unequal, it is inexact to the extent of the inequality. When these quotients are examined, there must be recollected that they consist of two sets—one for the uncoppered hull (columns *A*, *B*, *C*, *D* and *E*), the other for the coppered hull (columns *F* and *G*); consequently, the equality referred to must be looked for only between the experiments of each set.

The fact that in the experiments with the coppered hull, its resistance was less than when it was uncoppered, makes its speed higher with a given net power than when uncoppered; and the quotients of the division of the cubes of the speeds of the vessel by the net horses-power will consequently be higher with the coppered than with the uncoppered hull. These higher quotients (experiments *F* and *G*, line 24) divided into the corresponding utilizations, or quantities on line 23, give accordingly less results on line 25 for experiments *F* and *G* than for the remaining experiments.

The near equality of the quantities on line 25 for experiments *A*, *B*, *C*, *D* and *E* with the uncoppered hull, and for experiments *F* and *G* with the hull coppered, prove the principle and the constants employed in calculating the distribution of the power to be correct. No nearer approach to equality than these quantities present could be obtained from delicate laboratory experiments. With this confirmation, the data and results of these screw trials may be accepted as absolutely exact.

Results.

The experiments allow the difference between the resistance of the of the hull when coppered and when uncoppered to be determined. The mean speed of the five experiments *A*, *B*, *C*, *D* and *E*, with the uncoppered hull (line 2, Table No. 2) was 9·9741 geographical miles per hour; and the mean thrust of the screw, expressing the resistance of the vessel, was 1814·1283 pounds (line 4, Table No. 2). The mean speed of the two experiments *F* and *G*, with the hull coppered, was 9·8442 geographical miles per hour, and the mean thrust of the screw was 1612·0248 pounds. The latter quantity increased in the ratio of the squares of the speeds 9·9741 and 9·8442 geographical miles per hour, becomes 1654·8370 pounds, which compared with the 1814·1283

pounds, shows that leaving off the copper from the exterior immersed surface of the hull, increased its resistance 12·5372 per centum over its resistance when coppered.

The experiments also allow the determination to be made of the power of the speed in which the resistance of the hull increased for the interval between the extreme experimental speeds of 9·5346 (experiment *A*) and 10·4190 (experiment *D*) geographical miles per hour. For this purpose, the mean of the speeds and of the corresponding resistances of the hull in experiments *A*, *C* and *G*, in which the speed did not vary enough to sensibly influence the result, will be taken for one speed; the mean of the speeds and of the corresponding resistances of the hull in experiments *B*, *E* and *F*, in which the speed did not vary enough to sensibly influence the result, will be taken for another speed; and the speed and resistance of the hull in experiment *D* will be taken for the last speed. The resistances of the hull, line 4, in experiments *F* and *G*, during which the hull was coppered, will be equated for those in the remaining five experiments during which the hull was uncoppered, by multiplying them by the 1·125372 in the immediately preceding paragraph.

The mean speed of experiments *A*, *C* and *G* was

$$\left(\frac{9{\cdot}5346 + 9{\cdot}6264 + 9{\cdot}7493}{3} = \right) 9{\cdot}6368 \text{ geographical miles per hour;}$$

and the mean corresponding resistance of the hull was

$$\left(\frac{1650{\cdot}9925 + 1688{\cdot}6734 + 1580{\cdot}2261 \times 1{\cdot}125372}{3} = \right) 1706{\cdot}0027 \text{ lbs.}$$

The mean speed of experiments *B*, *E* and *F* was

$$\left(\frac{10{\cdot}0974 + 10{\cdot}1929 + 9{\cdot}9390}{3} = \right) 10{\cdot}0764 \text{ geographical miles per hour,}$$

and the mean corresponding resistance of the hull was

$$\left(\frac{1851{\cdot}0375 + 1909{\cdot}3913 + 1643{\cdot}8234 \times 1{\cdot}125372}{3} = \right) 1870{\cdot}1139 \text{ lbs.}$$

The speed of experiment *D* was 10·4190 geographical miles per hour, and the corresponding resistance of the hull was 1970·5470 pounds.

The experiments with the closely approximating speeds have been grouped and their mean results taken, because the greater the number of observations included the nearer accurate is the final determination.

From the above appears it that the resistance of the hull for the

increase of speed from 9·6368 to 10 0764 geographical miles per hour was in the ratio of the 2·0590 power of the speed. Had the resistance for the speed of 10·0764 miles been 1865·1949 pounds instead of 1870·1139 pounds, it would have increased exactly as the square of the speed; the difference of 4·9190 pounds is only 0·0026 per centum of the experimental resistance in excess.

There also appears from the above that the resistance of the hull for the increase of speed from 10·0764 to 10·4190 geographical miles per hour was in the ratio of the 1·5646 power of the speed. Had the resistance for the speed of 10·4190 miles been 1999·4435 pounds instead of 1970·5470 pounds, it would have increased exactly as the square of the speed; the difference of 28·8965 pounds is only 1·4664 per centum deficit of the experimental resistance.

Finally, there appears likewise from the above, that the resistance of the hull for the increase of speed from 9·6368 to 10·4190 geographical miles per hour was in the ratio of the 1·8472 power of the speed. Had the resistance of the speed of 10·4190 miles been 1994·1875 pounds instead of 1970·5470 pounds it would have increased exactly as the square of the speed; the difference of 23·6405 pounds is only 1·1997 per centum in deficit of the experimental resistance.

It is obvious that the slight differences found between the experimental resistances and what these should be according to the ratio of the squares of the speeds are so small that they may be fairly attributed to errors of observation and assumption. In fact, the data evidently cannot be relied on as to any greater accuracy, and the conclusion is, therefore, warranted that within the experimental speeds the resistance of the hull of the *Lookout* at different speeds varied as the squares of the speeds.

In experimenting with different propellers for the purpose of ascertaining their relative economic merits, that is to say, the relative per centage which they apply to the vessel propulsively of the power they receive from the engine, care should be taken that in every experiment the same speed of vessel be maintained, which equality eliminates all question as to the law of variation of resistance of the hull in function of its speed. If that resistance increase as the squares of the speeds then the accuracy of the results is not affected by variations of the vessel's speed because the slip of the same propeller would remain constant at all speeds, the reaction it obtains from the water varying always as the square of its revolutions in equal time. But if the resist-

ance of the vessel increase in a higher ratio than the square of the speed, then the slip of the same propeller would be proportionally greater at higher than at lower speeds of the vessel, and the propeller experimented with at higher speed would, relatively to another experimented with at lower speed, give a less economic result than if it were experimented with at the same speed. At the higher speed there would be proportionally more power expended in the slip, and consequently more power expended in overcoming the resistance of the water to the surfaces of the propeller moving in it. In other words, at the higher speed of vessel the same propeller will apply to it propulsively a less percentage of the power received from the engine than at the lower speed, if the resistance of the vessel increase in a higher ratio than the square of its speed.

Assuming, in the case of the experiments with the screws of the "Lookout," that the resistances of the vessel at the different speeds varied as the square of those speeds, an assumption warranted by the experimental results, the following would be the resistances of the coppered vessel for a speed of 10 geographical miles per hour, deduced from the experimental resistances at the experimental speeds in the ratio of the squares of these speeds to the square of 10:

Screw.	Resistance of vessel in pounds at 10 miles per hour.
A	1613·78
B	1613·24
C	1619·28
D	1613·02
E	1633·06
F	1664·06
G	1662·54
Mean,	1631·28

In the above determinations the experimental resistances with screws *A*, *B*, *C*, *D* and *E* have been reduced in the ratio of 1·125372 to 1, as the vessel in those cases was not coppered, that ratio expressing the effect of the coppering.

An examination of line 23, Table No. 2, shows that screws *B*, *C*, *D* and *E* had the highest economic efficiency; also, that their economic results were sensibly the same, as they all applied propulsively to the *uncoppered* vessel, 70 per centum of the net horses-power developed by the engine, that is to say, 70 per centum of the power applied to the

crank-pin. And, if the proper correction be made for the diminution of the vessel's resistance due to coppering, these screws would have usefully applied to the *coppered* vessel 71·3117 per centum of the net horses-power developed by the engine. Now, screws *B*, *C*, *D* and *E*, though having the same diameter and number of blades, varied largely in pitch and in the fraction used of the pitch, the pitches being 7·500, 7·500, 8·625 and 9 feet, and the respective fractions used of these pitches being 0·5283, 0·2703, 0·1994, 0·3549, showing that for a given diameter the pitch and fraction used of the pitch may vary considerably without affecting the economic performance of the screw. This arises from the fact that the losses of useful effect by the screw are, 1st, that which is due to its slip, and 2d, that which is due to the resistance of the water on the surface of its blades; including in 2d the direct resistance of the forward edges of the blades. And that the 1st loss cannot be diminished without the 2d loss being increased, *pari passu* and *vice versa*. The slip depends on the proportion of the acting surface of the screw in a given time, relatively to the resistance of the vessel, and can only be lessened by adding more of such surface, which, with a given diameter of screw, can be effected by either diminishing the pitch or increasing the fraction used of the pitch. But, as the loss by the resistance of the water on the screw surface is in direct proportion to that surface multiplied by the square of its velocity, any increase of the acting surface is necessarily accompanied by increased loss due to this cause. The two kinds of loss are found experimentally to be so balanced in nature that within very considerable limits whatever can be diminished of one kind is attended by an almost exactly equivalent increase of the other kind.

Screw *G* applied to the coppered vessel, and having the same diameter as screws *B*, *C*, *D* and *E*, with the pitch of 7·9 feet, which lies within the limits of their pitches, gave a utilization of 69·8750 per centum of the net power developed by the engine, instead of the utilization 71·5117 per centum, that would have been given by those screws had they been applied to the coppered vessel. This shows an inferiority of $\left(\frac{71{\cdot}3117 - 69{\cdot}8750 \times 100}{71{\cdot}3117} = \right)$ 2·0147 per centum for screw *G*, which is probably owing to the excessive fraction used of its pitch, namely, 0·5736, the largest fraction used in the cases of screws *B*, *C*, *D* and *E* being 0·5283. The increased power required to overcome the resistance of the water to the surfaces of screw *G*, owing to the increased

S 1880, IN THE POTOMAC RIVER, BY A BOARD OF UNITED STATES NAVAL
ERTAINING THE BEST PROPORTIONS OF SCREW FOR THAT VESSEL.

er been reduced in the ratio of the net area of the piston of the large cylinder to
ters at the head of the columns in this table designate the same experiments as are

		Hull coppered.	
D	*E*	*F*	*G*
April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
10·4190	10·1929	9·9390	9·7493
20·1542	17·9739	21·9620	12·7150
1970·5470	1909·3913	1643·8234	1580·2261
153·4601	140·0500	143·5402	143·4125
25·8904	26·8090	23·4011	21·3831
2 7164	2·7164	2·7164	2·7164
23·1740	24·0926	20·6847	18·6667
1·7380	1·8069	1·5514	1·4000
1·0914	1·7154	1·4241	2·3233
4·1003	3·6973	3·8893	1·9000
16·2443	16·8730	13·8199	13·0434
100·5812	95·0489	85·0341	77·6320
10·5529	9·6307	9·8707	9·8620
90·0283	85·4182	75·1634	67·7700
6·7521	6·4064	5·6373	5·0827
4·2398	6·0816	5·1748	8·4348
15·9291	13·1084	14·1328	6·8982
63·1073	59·8218	50·2185	47·2543
7·5000	7·5000	7·5000	7·5000
4·7094	7·1198	6·8847	12·4462
17·6934	15·3462	18·8028	10·1788
70·0972	70·0340	66·8125	69·8750
12·5632	12·3978	13·0624	13·6733
5·5796	5·6408	5·1149	5·2193

SCREW EXPERIMENTS.

By Chief Engineer ISHERWOOD, U. S. Navy.

TABLE No. 2, Containing the Distribution of the Piston Pressures and of the Indicated Horses-Power during the Experiments made in 1880, in the Potomac River, by a Board of United States Naval Engineers with the Different Screw Propellers Applied to the Fish Commission's Steamer "Lookout," for the Purpose of Ascertaining the Best Proportions of Screw for that Vessel.

The piston pressures in this table are the pressures that would have been upon the piston of the large cylinder had the pressures upon the piston of the small cylinder been reduced in the ratio of the net area of the piston of the large cylinder to the net area of the piston of the small cylinder, and the quantities thus obtained added to the observed pressures on the piston of the large cylinder. The capital letters at the head of the columns in this table designate the same experiments as are given in the columns of Table No. 1, with the same letter headings.

No. of Line.			Hull not coppered.					Hull coppered.	
			A	B	C	D	E	F	G
1		Date of experiments,	January 19, 20 & 21.	March 26, 29 & 30.	January 7, 10 & 15.	April 1, 2 & 3.	June 23, 26 & 28.	October 19 & 20.	September 24 & 25.
2		Speed of the vessel per hour, in geographical miles of 6086 feet,	9·5346	10·0974	9·6264	10·4190	10·1929	9·9390	9·7493
3		Slip of the screw, in per centum of its speed,	24·4072	13·6772	18·0439	20·1542	17·9739	21·9620	12·7150
4		Thrust of the screw, in pounds,	1650·9925	1851·0375	1688·6734	1970·5470	1909·3913	1643·8234	1580·2261
5		Double strokes of engine's pistons, and revolutions of the screw made per minute,	152·3085	158·1988	158·8560	153·4601	140·0500	143·5402	143·4125
6	Distribution of the indicated pressure on the piston.	Indicated pressure on the piston, in pounds per square inch,	21·9103	23·0038	20·4793	25·8904	26·8090	23·4011	21·3831
7		Pressure required to work the engine, *per se*, in pounds per square inch of piston,	2·7164	2·7164	2·7164	2 7164	2·7164	2·7164	2·7164
8		Net pressure applied to the crankpin, in pounds per square inch of piston,	19·1939	20·2874	17·7629	23·1740	24·0926	20·6847	18·6667
9		Pressure absorbed by the friction of the load, in pounds per square inch of piston,	1·4395	1·5216	1·3322	1·7380	1·8069	1·5514	1·4000
10		Pressure expended in overcoming the resistance of the water to the surface of the screw blades, in pounds per square inch of piston,	1·1537	2·2522	1·2704	1·0914	1·7154	1·4241	2·3233
11		Pressure expended in the slip of the screw, in pounds per square inch of piston,	4·0518	2·2586	2·7355	4·1003	3·6973	3·8893	1·9000
12		Pressure expended in the propulsion of the vessel, in pounds per square inch of piston,	12·5489	14·2550	12·4248	16·2443	16·8730	13·8199	13·0434
13	Distribution of the indicated horses-power. Absolute.	Indicated horses-power developed by the engine,	84·4804	92·7090	82·3573	100·5812	95·0489	85·0341	77·6320
14		Horses-power expended in working the engine, *per se*,	10·4737	10·9475	10·9239	10·5529	9·6307	9·8707	9·8620
15		Net horses-power applied to the crankpin,	74·0067	81·7615	71·4334	90·0283	85·4182	75·1634	67·7700
16		Horses-power absorbed by the friction of the load,	5·5505	6·1321	5·3575	6·7521	6·4064	5·6373	5·0827
17		Horses-power expended in overcoming the resistance of the water to the surface of the screw blades,	4·4483	9·0767	5·1089	4·2398	6·0816	5·1748	8·4348
18		Horses-power expended in the slip of the screw,	15·6225	9·1025	11·0008	15·9291	13·1084	14·1328	6·8982
19		Horses-power expended in the propulsion of the vessel,	48·3854	57·4502	49·9662	63·1073	59·8218	50·2185	47·3543
20	Proportional.	Per centum of the net horses-power applied to the crankpin, absorbed by the friction of the load,	7·5000	7·5000	7·5000	7·5000	7·5000	7·5000	7·5000
21		Per centum of the net horses-power applied to the crankpin, expended in overcoming the resistance of the water to the surface of the screw blades,	6·0107	11·1014	7·1520	4·7094	7·1198	6·8847	12·4462
22		Per centum of the net horses-power applied to the crankpin, expended in the slip of the screw,	21·1096	11·1330	15·4001	17·6934	15·3462	18·8028	10·1788
23		Per centum of the net horses-power applied to the crankpin, expended in the propulsion of the vessel,	65·3797	70·2656	69·9479	70·0972	70·0340	66·8125	69·8750
24		Relative economic efficiencies of the screws, expressed by the quotient of the division of the cubes of the speeds (line 2) by the net horses-power applied to the crankpin (line 15),	11·7122	12·5049	12·4880	12·5632	12·3978	13·0624	13·6733
25		Quotients of the division of the quantities on line 23 by those on line 24,	5·5822	5·6190	5·6012	5·5796	5·6408	5·1149	5·2193

fraction used of its pitch over the largest fraction used with screws *B*, *C*, *D* and *E*, was probably greater than the lessened power expended in the lessened slip consequent on the use of that greater fraction of the pitch.

Screw *A* applied to the uncoppered vessel, and screw *F* applied to the coppered vessel, having very nearly the same diameter and fraction used of the pitch, but pitches of respectively 8·4 and 9·0 feet gave the same utilizations when that of screw *A* is corrected for the greater resistance of the uncoppered vessel, showing the difference of the pitches not to have affected the economic results. The utilizations of screws *A* and *F*, when equated for the resistance of the coppered vessel, are 66·8 per centum of the net power developed by the engine, consequently these two screws of $4\frac{2}{3}$ feet diameter are

$\left(\frac{71{\cdot}3117 - 66{\cdot}8000 \times 100}{71{\cdot}3117} = \right)$ 6·3267 per centum inferior to the four

screws *B*, *C*, *D* and *E* of 5 feet diameter, the pitches and fractions used of the pitch of the former lying within the limits of those of the latter. This inferiority is caused wholly by the smaller diameter, whereby the loss of useful effect, due to the greater slip resulting from the less acting surface of screws *A* and *F*, more than counterbalances the gain due to the less resistance of the water to the less acting surface of those screws. The utilizations of screws *A* and *F* could doubtless have been much increased had the fraction used of their pitch been increased from about one-third, which it was, to one-half; or, had their pitch been decreased, retaining the same fraction of pitch.

The quantities on line 24 of Table No. 2 shows relatively the utilizations of the different screws, when the comparison is made directly by dividing the cubes of the speeds (line 2) by the net horses-power developed by the engine (line 15). The quantities on line 24 are the quotients of such divisions, and have very nearly the same proportions to each other as the quantities on line 23—considering the experiments with the uncoppered vessel separately from those with the vessel coppered—as appears from the quantities on line 25, which are the quotients of the division of those on line 23 by those on line 24. The equality of the latter quotients proves, 1st, that the distribution of the power in the different experiments as given in Table 2, lines 13 to 23, both inclusive, is correct; and 2d, that no difference in the utilizations results from the difference in the metals, cast-iron and cast-brass, of which the screws were made. The only difference that

could have resulted would have been due to the different surface textures of these two metals, whereby the resistance of the water per unit of their surface might have been different.

These experiments show what many others have shown when properly interpreted—that the utilizations of screws of different proportions of diameter, pitch and fractions used of the pitch, and of different outlines and inclinations of the blades applied to the same vessel, vary very little unless extreme variations of dimensions are resorted to. The outline and inclination of the blades appear to be absolutely without effect upon the utilization, which is controlled entirely by the diameter, pitch and fraction used of the pitch.

The experiments show also that the method of distributing the indicated horses-power developed by the engine, originally devised and long practiced by the writer, is correct, and that by its means the important question of the resistance in pounds of the hull, *per se*, of a vessel at a given speed may be ascertained indirectly quite as accurately as by the direct application of a dynamometer.

The true performance and distribution of the power of the *Lookout* with her bottom coppered, when propelled by screw *E*, deduced from all the experiments, is as follows for the speed of 10 geographical miles per hour, the slip of the screw being 16 per céntum of its axial velocity (product of revolutions and pitch); the number of its revolutions, 134·1711 per minute; the indicated pressure on the piston of the large cylinder, 23·4175 pounds per square inch, that cylinder being supposed to be used alone; the thrust of the screw or resistance of the hull, 1631·28 pounds, as herein determined; the indicated horses-power developed by the engine, 79·5393, and the pressure required to work the engine, *per se*, 2·7164 pounds per square inch of the piston of the large cylinder alone.

Distribution of the Power.

	Horses-power.		Per cent. of net horses-power.
Indicated power developed by the engine,	79·5393		
Power expended in working the engine, *per se*,	9·2265		
Net power applied to the crank-pin,	70·3128	or	100·0000
Power absorbed by the friction of the load,	5·2734	"	7·5000
Power expended in overcoming the resistance of the water to the surface of the screw blades,	5·3474	"	7·6051
Power expended in the slip of the screw,	9·5507	"	13·5832
Power expended in the propulsion of the vessel,	50·1413	"	71·3117
Totals,	70·3128	or	100·0000

DISCUSSION

Of the Papers of C. P. Sandberg on "Rail Specifications and Rail Inspection in Europe," of C. B. Dudley on the "Wearing Capacity of Steel Rails in Relation to their Chemical Composition and Physical Properties," and of A. L. Holley on "Rail Patterns," at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17th, 1881.

(Continued from page 36.)

William Metcalf, Pittsburg, Pa.: In rising to discuss Dr. Dudley's paper, I feel somewhat as I did at the Baltimore meeting—that a "crucible" man has no right to interfere in a "Bessemer" discussion; yet having read the paper very carefully, I feel impelled to say something, for two reasons: First, because I believe Dr. Dudley is entirely on the right track, and having undertaken and partly accomplished a great work, he is entitled to the help of all who have experience in these matters; and second, because the data given force me to concur in Captain Jones' opinion that the analyses are incomplete, since they ignore sulphur, copper, nitrogen, and possibly other injurious elements.

In an experience of fourteen years, and with probably more than a hundred tests, we have never found the chemistry and the physics of crucible steel to disagree. If in any case a disagreement has appeared, it has been our invariable custom to go all over our physical tests with great care, and if we found no error, then to refer the matter back to the chemist, who has invariably found some unexpected element to account for the trouble.

It is only just to the chemist to say here, that ordinarily he is only expected to determine phosphorus, silicon and sulphur. Generally the metals, with the exception of manganese, are not looked for, although a watch is usually kept for copper and arsenic. Further, in most cases we have found our own work more liable to be faulty than the chemist's.

Having arrived then at such a degree of experience that we can predict the analysis from our tests, or our tests from the analysis, with almost absolute certainty, I can see no reason why the same results may not be attained in the Bessemer practice. But two things are

essential, neither of which we have here; first, complete analyses; and second, a record of the nature of the blow, the heat at which the ingots were bloomed, and the rails finished,—in short, a complete history of the manufacture.

This latter is quite as essential as accurate and complete analyses. Dr. Dudley ignores sulphur and copper on fair enough grounds commercially speaking, but when he announces so grave a conclusion as he has reached, in a scientific way, the omission of any elements that may affect the conclusion is hardly justifiable. His differences of phosphorus units, which I must term units of rottenness as far as phosphorus is concerned, and which I am sorry he did not name "*Units of Alloys*,"—are very small, and if sulphur and copper had been included they might have upset his conclusions altogether. The omission of nitrogen is not to be criticised in the same way, because it has not been usual to regard nitrogen in testing steels, yet I am forced to believe that nitrogen plays a very important part in Bessemer steel.

We had occasion recently to test some of the finest Bessemer steel that is made, in order to ascertain how far the Bessemer people were encroaching on the domain of the crucible steel manufacturers. In the accompanying table are the analyses of the Bessemer steel referred to, two samples of the very best foreign crucible steel, and one of the best American crucible steel.

Kinds of steel.	Carbon.	Phosphorus	Silicon.	Sulphur.	Manganese.	Copper.
Bessemer	0·400	0·027	0·003	trace.	....	....
Foreign crucible	1·295	0·020	0·009	trace.	0·050	....
	0·862	0·005	0·007	none.	....	0·0013
American crucible	0·960	0·021	0·033	trace.	0·077	....

All three of the crucible steels were of exceptionally good quality. It will be observed that, according to the analysis, the Bessemer steel should have been equally good. Upon a careful test of an 0·80 carbon billet it proved to be thoroughly worthless.

The case was then referred to Professor Langley, and he attributed the trouble to oxygen, and predicted that if we would melt a 0·40 carbon billet with a little ferromanganese to remove the oxygen and bring up the carbon to 0·80, and also give a little more silicon which the steel would take from the crucible, that we would have a steel equal to the others given above. To be sure of our work, we melted

the billet, the analysis of which is given above, and produced an ingot of about 0·80 carbon, as near as the eye could determine, and that is within ·03 in such high steels. The remelted Bessemer steel was just as bad as the other, and of this we assured ourselves by the most careful and repeated tests.

Now we know oxygen was not the cause of the fault, for if it had been, the ferromanganese would have removed it. In the Bessemer manufacture immense volumes of nitrogen are blown through the molten mass, and by the evidence of all the most eminent chemists who have examined the subject, we know that nitrogen does unite with iron, that the compound is brilliantly lustrous, hard, brittle, and even friable, and that it will harden like steel. We also know that there is a peculiar lustre in all Bessemer steel, which makes it easily distinguishable by an expert from crucible or open-hearth steel, provided none of them have been overheated. Is it not more than probable, then, that nitrogen is entitled to far more serious attention than it has yet received?

Dr. Dudley classes carbon with phosphorus, silicon, and manganese in making up his units, and he may be correct in his make-up in his relative values of each, but I cannot see how he has proved his formula, nor how it is to be disproved with our present knowledge.

While maintaining that carbon in steel is the great friend of the manufacturer and the only fit alloy of iron, I must admit that when it is present in quantity with phosphorus, silicon, manganese, or sulphur in quantity, it vitalizes and makes more active all of the bad qualities of these elements, and therefore if Dr. Dudley must have the quantities of these elements in his rails which he permits in his formula, he is quite right in saying that the softest rails ought to wear the best. In regard to the wearing of wire dies we have found

Carbon,	Tungsten,
1·37.	0·78.

too soft, *i. e.*, it wore too easily. Also steel of the composition

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
1·7	0·20	0·091	0·387	0·02

was too soft. The best foreign dies contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2·89	0·14	0·131	0·26	0·02

and equally good and entirely satisfactory home-made steel contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2·37	0·20	0·091	0·18	0·03

In this case good wear depends upon high carbon.

In steel rolls, in which we have had some experience, we have found that very soft rolls of about 0·30 carbon wore too fast from excessive flow, taking the shape shown in the accompanying cut, the overflow sometimes amounting to more than half an inch. This cuts away the brasses and involves much redressing. Rolls of about 0·70 carbon made from the same iron, neither flow nor crack to any serious extent, and will do two to three times as much work as the softer rolls.

We know that steel flows under pressure, that the milder the steel the easier it will flow, and the easier it will shear; yet in the paper under discussion flow is disregarded entirely, although there is plain evidence of it in many of the sections given. Low shearing stress is advised as conducive to high wear, while it would seem plain that chilled flanges must act as admirable shears to trim off "flowed" edges.

Dr. Dudley says low phosphorus units ought to give the best wear, and so they ought. He proceeds to prove it by grouping the 32 rails of least wear, and the 32 rails of most wear in two groups, and taking a mean of their analyses. By a happy accident this mean sustains his view, or perhaps it would be fairer to say, from this accident he draws his conclusions. These groups consist of all sorts of rails, of different make, different conditions of wear and different tonnages. Would it not be fairer to compare rails from the same part of the track, subjected to the same conditions of wear, of the same tonnage, and presumably of the same make? Arranging the 64 rails under consideration in this way by means of the history given, I find 30 groups, consisting of twos, threes, and fours. Comparing the phosphorus units and wear per million tons as given in the table, I find that in twelve groups the softer rails have shown the least wear, while in eighteen groups the harder rails show the least wear.

This gives 18 against Dr. Dudley to 12 for him, and it seems as if this mode of comparison were the fairer, if the presumption is correct, that the rails of these different groups were of the same make in each group, because in

such case the chances are that the physical conditions, due to modes of manipulation in manufacture, were more nearly the same than they could possibly be in the more general average given in the table.

I would like to ask Dr. Dudley to relieve us, if he can, of a term which he has introduced into his papers and specifications, and which, for the sake of harmony, ought, I think, to be obliterated. I refer to the word "hardener." This is a term properly applied to any substance contained in steel, for anything mixed with iron will make it harder. But carbon is the great "hardener," and produces all of the wonderful and useful properties in steel with which we are familiar, and of the nature of which we know so little. Carbon, then, ought to to be distinguished as *the* hardener, and all other components should be known by some other name. If not, we shall have quack steelmakers coming out with more silicon steel, phosphorus steel, sulphur steel, and the like. And why not? Are they not all hardeners, and is any one better than another?

This is a serious matter, for we have too much duplication of meanings now. For instance, if you go to a founder and ask if he has a chill to make a certain size of die or roll, he asks, in reply, how much chill you want, and you tell him, say, half an inch or an inch, as the case may be. Then he asks if you want a tough chill, or a mild chill, or a hard chill. Would not an outsider be utterly puzzled to know what a chill was?

Again, a steelmaker talks of temper, and refers to steel of 0·30, 0·40, or 1·00 carbon, as it may happen; and a steel-user talks of temper, and means a yellow, or brown, or blue color, left on his steel after it is tempered. I once traveled many hundreds of miles to see about a steel trouble. A buyer had sent back a shear-knife which would not cut. Not waiting till the knife was received, I started off, and asked my partner to telegraph me his opinion after he had received the blade. We were both sure it had been burned. After my arrival on the scene and finding a man in real trouble, and a great temper, a message came in these words, "Temper too high; will send another bar." Greatly pleased, and thinking my way plain, in an evil moment I showed the message to my troubled friend, who interpreted it to mean that he had improperly treated the steel, and the indignation created was as profound as it was unexpected.

Who, then, can define steel? An International Committee of this Institute and many others have wrestled with the question, and yet

to-day there is a heavy suit pending in the United States courts, all turning upon the question whether steel is steel or iron.

Now we are threatened with the war of the "hardeners," and the contemplation of another complication in our nomenclature is no joke for the steel makers; and in their behalf I appeal to Dr. Dudley to relieve us before it is too late.

In conclusion, allow me to say that I hope my remarks will not be regarded as antagonistic to Dr. Dudley's great work, for I could have no possible reason for entering the lists as an antagonist; and, on the contrary, I decidedly agree with him that the chemistry of steel is an important factor in its manufacture, and chemistry and physics must go together if results of any value are to be reached; and I firmly believe that eventually all engineers will know how to make and to appreciate chemical specifications. I therefore feel that we should all contribute all we know or think on the subject, and I hope these remarks will be considered a thoroughly sympathetic criticism.

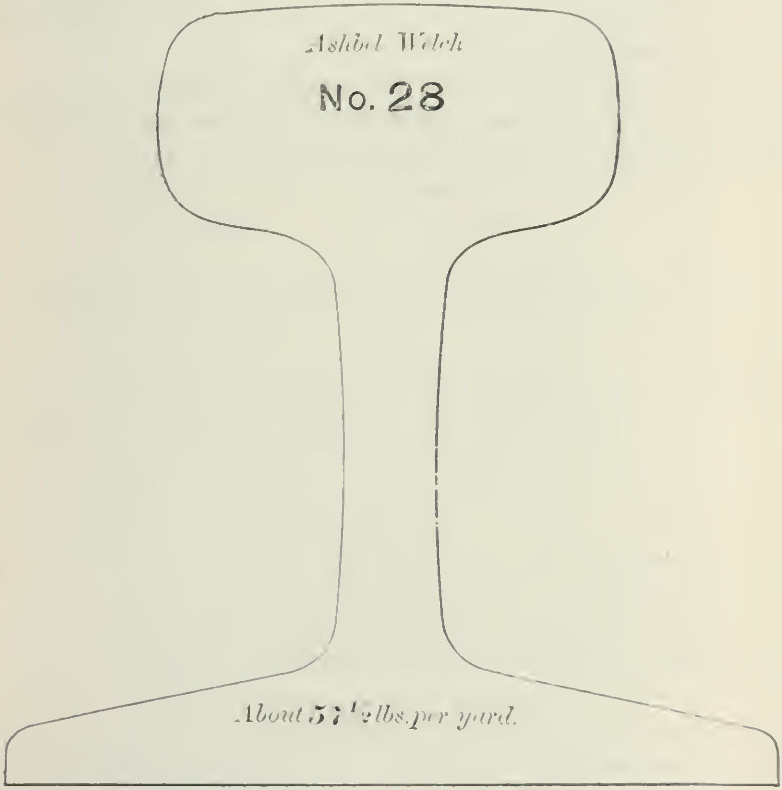
Letter from William R. Hart.

I was this morning an interested listener to the remarks of Mr. Ashbel Welch in regard to his designing a new section for steel rails, in 1866; and for the sake of the truth of history, and in order to give the credit to an American (and where it rightfully belongs) of designing the first section for steel rails, which was intelligently adapted to that material, I beg to state the following facts:

On the 14th of August, 1866, Mr. Ashbel Welch gave me an order for 200 tons of steel rails, to be made for the Camden and Amboy railroad, from a section designed by him. A copy of this section I append herewith.

Messrs. John Brown & Co., of Sheffield, for whom we were then agents, at first declined to roll these rails, owing to the thinness of the flange, but subsequently accepted this order. In principle, as I think you will see from the section, this pattern was similar to what is now known as the Sandberg section, having a large amount of metal in the head, and without superfluous weight in the stem and base. This section was put upon our book, with the title "Ashbel Welch section;" and this name was also rolled upon the stem of the rails. We sold afterward large quantities of these rails under this title to the Philadelphia and Baltimore Railroad Company, as well as to the Camden and Amboy Railroad Company.

I make this statement in justice to Mr. Welch, who ought to have the credit of designing a section which had much to do with making steel rails a success. At the time Mr. Welch designed this section it



was quite a new departure, and as our old section-book shows it was very different from any of the sections which up to that time had been ordered by any of the railroad companies.

I shall be very glad if this fact can be recorded upon the minutes of the Institute, and remain,

Yours very truly,

WILLIAM R. HART.

PHILADELPHIA, Feb. 17th, 1881. *Agent, Naylor & Co.*

LETTER FROM R. H. SAYRE, BETHLEHEM, PA.

. . . . . The subject is one of great interest in every point of view to railroad managers and steel-rail makers. It has occurred to

me that if in this connection your society would take up the matter of the shape or section of steel rails and the form of joint, and be able to arrive at such form for different weight of rails as you could recommend to the railroads of this country with a view of obtaining uniformity, it would be, in case of adoption, of great value both to the railroads and makers of rails.

My idea is that if both your society and that of the civil engineers should join in the adoption of templates and their recommendation, it would be more likely to have the desired effect.

Yours truly,

ROBERT H. SAYRE.

WILLIAM KENT, Pittsburgh, Pa.:—The steel manufacturers of this country must ever be grateful to Dr. Dudley for his painstaking and conscientious endeavor to establish the relation between the chemical analysis and the wearing capacity of steel rails. They must thank him for the vast array of facts he presents, and especially for having given them sixty-four analyses with which to combat his own conclusions and to establish their own, which are entirely opposite to his.

In Dr. Dudley's discussion of his former investigation, at the Pittsburgh meeting, he said, "If you do not like my conclusions, draw your own conclusions." I have studied his last paper as thoroughly as the limited time since I received it would admit, and have drawn some conclusions which I will first state, and then attempt to demonstrate.

Briefly stated, my conclusions are: 1st, That as far as these 64 analyses reveal anything of service to railmakers and consumers they do reveal, or seem to reveal, that within the following chemical and physical limits, viz.:

Carbon,	0·20 to 0·60
Phosphorus,	0·026 to 0·145
Silicon,	0·015 to 0·480
Manganese,	0·252 to 0·880
Phosphorus units,	20·3 to 5·72
Bending weight,	2270 lbs. to 4260 lbs.
Deflection,	13° to 190°

or in other words within the limits of nearly the whole range of the chemical and bending tests of these 64 rails, the wearing capacity bears no relation at all to carbon, to phosphorus, to silicon, to manga-

nese, to phosphorus units, to bending weight or to deflection; or if there is any relation between the wearing capacity and these six or seven variables, it is so obscured by the action of other causes or variables not yet known, that such relation cannot be expressed by any practical formula.

2d. That the difference in wearing capacity of these 64 rails was not due to carbon, to phosphorus, to silicon, to manganese, or to any combination of these four elements, but that it was due to some other cause or combination of causes, of which Dr. Dudley's whole investigation furnishes us no clue whatever. A few of the many possible causes I may name, sulphur, copper, oxide of iron, inclosed air or other gases, overblowing, underblowing, overheating, underheating, too hot-finishing, too cold-finishing, cold-straightening, too great or too little reduction from the rail to the ingot, or the portion of the ingot from which the rail was taken, as top or bottom. I have no idea which of these causes has the greatest influence in determining the wearing capacity of a rail,—probably no one else has,—but I firmly believe that some one or more of them has far more influence than all the four chemical elements named in Dr. Dudley's analyses within the limits which I have mentioned.

3d. That the railroad companies must utterly abandon, for the next ten years at least, the attempt to limit rail manufacturers to certain prescribed chemical analyses, unless within the wide range of analyses I have given above. If they would seek to establish a definite specification by which to insure good wearing capacity, and at the same time not make the specification an impracticable one for railmakers to meet, as Dr. Dudley's certainly is, they must inaugurate another investigation (and I know of no one so well fitted to undertake it as Dr. Dudley himself, after he shall have thoroughly disabused his mind of conclusions already formed), which investigation shall take at least ten years to complete, and shall include not only the effect of the four chemical elements now under discussion, but all the other supposed causes of difference in wearing capacity which I have mentioned above, besides many others I have not even thought of. Such an investigation I believe the railmakers would not object to; they should rather contribute towards it. It would richly repay the railroad companies by enabling them to secure better rails, providing that after the investigation was concluded it should reveal the causes of defective wearing capacity, which Dr. Dudley's present investigation does not do.

4th. That Dr. Dudley's failure to establish, after years of careful investigation, the relation between the chemical analysis and the wearing capacity of a rail, should be a lesson to other consumers of steel besides the railroad companies, not to attempt to regulate the steel manufacturer by a chemical specification based upon their own investigations, far less elaborate than that of Dr. Dudley or that of the manufacturers. It is a common occurrence for a steel manufacturer to be asked to guarantee both a chemical and a physical specification entirely inconsistent with each other, and one or both impossible of fulfilment. The attempt to fill orders with such specifications is not only annoying but often even ruinous to the manufacturer, causing the rejection of much steel that is even better adapted to the wants of the consumer than some of the steel which is accepted. This is precisely the result which would happen if Dr. Dudley's rail specifications were to be insisted on.

I will now undertake to give a reason for the conclusions I have drawn. Suppose that the Pennsylvania Railroad Company were at once to insist that all rails furnished them should conform to Dr. Dudley's specifications in these six particulars:

Carbon, ...	·25 to ·35
Phosphorus, . . .	not over ·10
Silicon, ...	not over ·04
Manganese, . . .	·30 to ·40
Bending weight, . . .	not over 3000 lbs.
Deflection, . . .	not under 130°

Suppose that before the rail manufacturers had "got the hang" of making rails within these rigid limits, and while they were yet making them with the wide range of composition which they are now doing, I should be sent as inspector to one or more works to inspect 64 lots of rails, one rail being taken out of each lot for test, the test rail being supposed to represent exactly the lot from which it was taken. Suppose, further, that I test these 64 rails chemically and physically, and find that the results are exactly those given in Dr. Dudley's tables. I tabulate the results as I have done here, rejecting all the tension and torsion tests, as these are not included in my instructions, and I carefully examine the table to see how many I should accept and how many I should reject. How many of these 64 rails or lots would I have to accept under these specifications? *Only three!* And one of

these three would be classed by Dr. Dudley as a bad rail, standing number 10 out of the 16 rails marked "Level Tangent."

I would reject *sixty-one* of these sixty-four rails (or lots) for violation of from one to all six of the specifications.

34	for too	great bending load.
29	"	small deflection.
42	"	high or too low carbon.
31	"	high phosphorus.
32	"	high silicon.
52	"	high or too low manganese.

As Dr. Dudley classes the 64 rails into two grand divisions, viz., 32 slower wearing and 32 faster wearing (I call them here good rails and bad rails for the sake of brevity), I have to reject 30 of his *good rails.*

13	for too	great bending load.
7	"	small deflection.
21	"	high or too low carbon.
11	"	high phosphorus.
14	"	high silicon.
21	"	high or too low manganese.

On the tables (Plates 6 and 7)* I have marked each cause of rejection with a black mark. You will notice that they seem covered all over with black marks, that their position follows no regular law, but that I seem to have distributed them with commendable impartiality. I count the black marks, and find the total causes of rejection to number 210 out of a possible 366, or an average of 3·44 for each of the 61 rails rejected. Of the 31 bad rails rejected the black marks or causes of rejection number 123, an average of 3·97. Of the 30 good rails rejected the causes number 87, an average of 2·9. Here is a point in favor of Dr. Dudley: the 31 bad rails rejected do have a worse chemical and physical record than the 30 good rails rejected, the average number of black marks against the bad rails being greater in the ratio of 3·97 to 2·9. But his gratification in this regard must be somewhat diminished when he learns that by selecting a number of very, *very* bad rails, which I have done on the tables, drawing heavy lines around them, and marking them *worst rails*, viz., the two worst rails out of 16 marked tangent grade, the four worst rails out of 8 marked curve

* See JOURNAL for March, 1881.

grade, low side; the three worst out of 8 curve grade, high side; the three worst out of 16 level tangent, the two worst out of 8 level curve, low side, and the four worst out of 8 level curve, high side, the total causes of rejection of these 18 rails is 68, or an average of only 3·78.* That is, the 18 worst rails have fewer average causes of rejection than the whole 31 rejected rails designated "faster wearing" in the ratio of 3·78 to 3·97.

In Dr. Dudley's table of averages he compares the chemical and physical properties of 32 slower-wearing rails with those of 32 faster-wearing rails in all conditions of service. The comparison agrees with his conclusion, except in the matter of silicon, the average silicon of the bad rails being lower than that of the good rails. Let us carry the comparison a little further, and compare the properties of the 18 worst rails, which I call very, *very* bad rails, with those of Dr. Dudley's 32 bad rails, and see whether it strengthens his position. Here is the comparison:

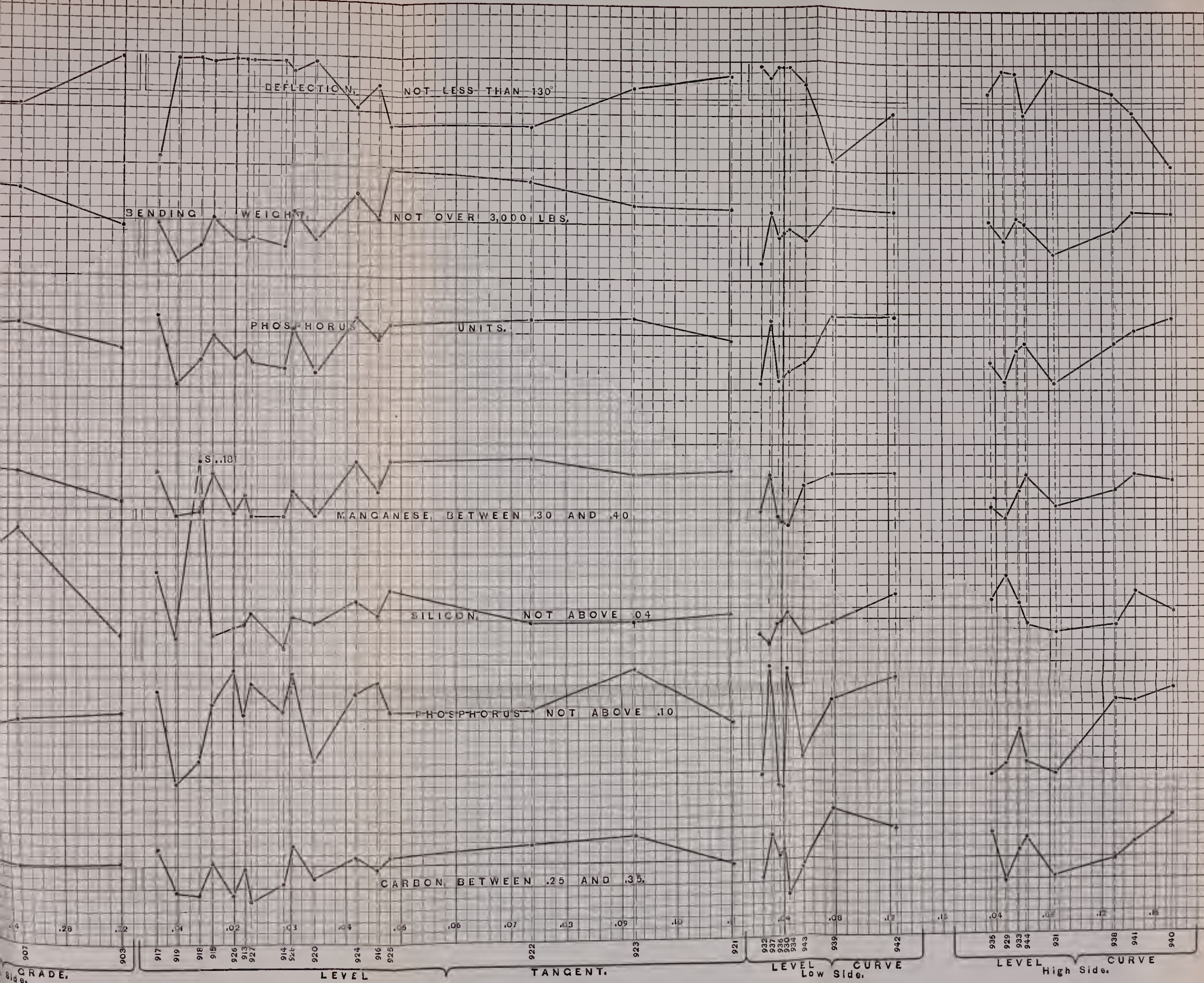
	Loss per million tons.	Bending weight.	Deflection.	C.	P.	Si.	Mn.	Phos. units.
32 good rails,	·0506	2878	160°	·334	·077	·060	·491	31·3
32 bad rails,	·1028	3222	133°	·390	·106	·047	·647	38·9
18 worst rails,	·1326	3209	135°	·412	·109	·040	·677	40·4
13 bad (?) rails,	·0668	3267	136°	·369	·105	·050	·632	37·9

The 18 worst rails show a greater loss per million tons than the 32 bad rails by 29 per cent., while in three elements of Dr. Dudley's specifications, viz., bending weight, deflection and silicon, they are slightly better, according to his ideas; and in three others, viz., carbon, phosphorus and manganese, slightly worse. If Dr. Dudley's conclusions were correct, we should expect to find that a lot of rails having 29 per cent. worse-wearing capacity than a larger lot which included them, would show a very marked deviation in average analysis and bending tests from the average analysis and bending tests of the larger lot; but actually the deviation is so slight that Dr. Dudley himself could not tell, if the results of tests alone were presented to him, which lot had the greater and which the less wearing capacity.

But I have carried the comparison still further. In the 4th line in the table I have placed the record of the other 13 bad (?) rails, out of

* The method of selection of these 18 rails is not an arbitrary one, as will be seen further on. Of the 16 level tangent rails, 14 should be classed as good and 2 as bad, a fairer classification than that of 8 faster and 8 slower wearing.


DEFLECTION
BENDING WEIGHT
PHOSPH. UNITS
MANGANESE
SILICON
PHOSPHORUS
CARBON
Si. .480.
Si. .18
DEFLECTION. NOT
BENDING WEIGHT. NOT
PHOSPHORUS
MANGANESE
Loss per Million Tons
Number of Rail
TANGENT. GRADE.
CURVE GRADE. Low Side.
CURVE GRADE. High Side.
LEVEL


DEFLECTION, NOT LESS THAN 130°
BENDING WEIGHT, NOT OVER 3,000 LBS.
PHOSPHORUS UNITS.
S. .18
MANGANESE, BETWEEN .30 AND .40
SILICON, NOT ABOVE .04
PHOSPHORUS NOT ABOVE .10
CARBON, BETWEEN .25 AND .35.
907 903 917 919 918 915 926 913 927 914 928 920 924 916 925 922 923 921
932 937 936 930 934 943 939 942
935 929 933 944 931 938 941 940
GRADE.
Side.
LEVEL
TANGENT.
LEVEL CURVE
Low Side.
LEVEL CURVE
High Side.

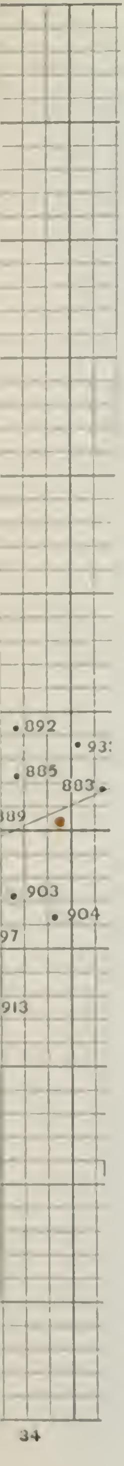
the 31 faster-wearing rails, which would have been rejected under Dr. Dudley's specifications. (I have omitted in this average rail No. 920, classed by Dr. Dudley as a bad rail from its faster wearing, but which conforms to his specifications in every respect.) I have placed an "?" after the word bad in designating these rails, as I think they should have been classed with the 32 good rails, on account of their wearing capacity being much nearer to that of the good rails than to that of the 18 worst rails with which they are associated. Comparing the 18 worst rails with the 13 bad (?) rails, we find the former to be over 98 per cent. (or nearly double) worse in wearing capacity than the former, while their analyses and bending-tests indicate them to be very nearly alike. The worst rails are lower in bending weight and silicon, and only slightly lower in deflection, and slightly higher in carbon, phosphorus and manganese. Here is a very plain case selected from Dr. Dudley's own work. Two distinct lots of rails, one lot twice as good in wearing capacity as another, but both lots closely agreeing in average analysis and bending tests. What stronger evidence could be produced of the fallacy of his formula?

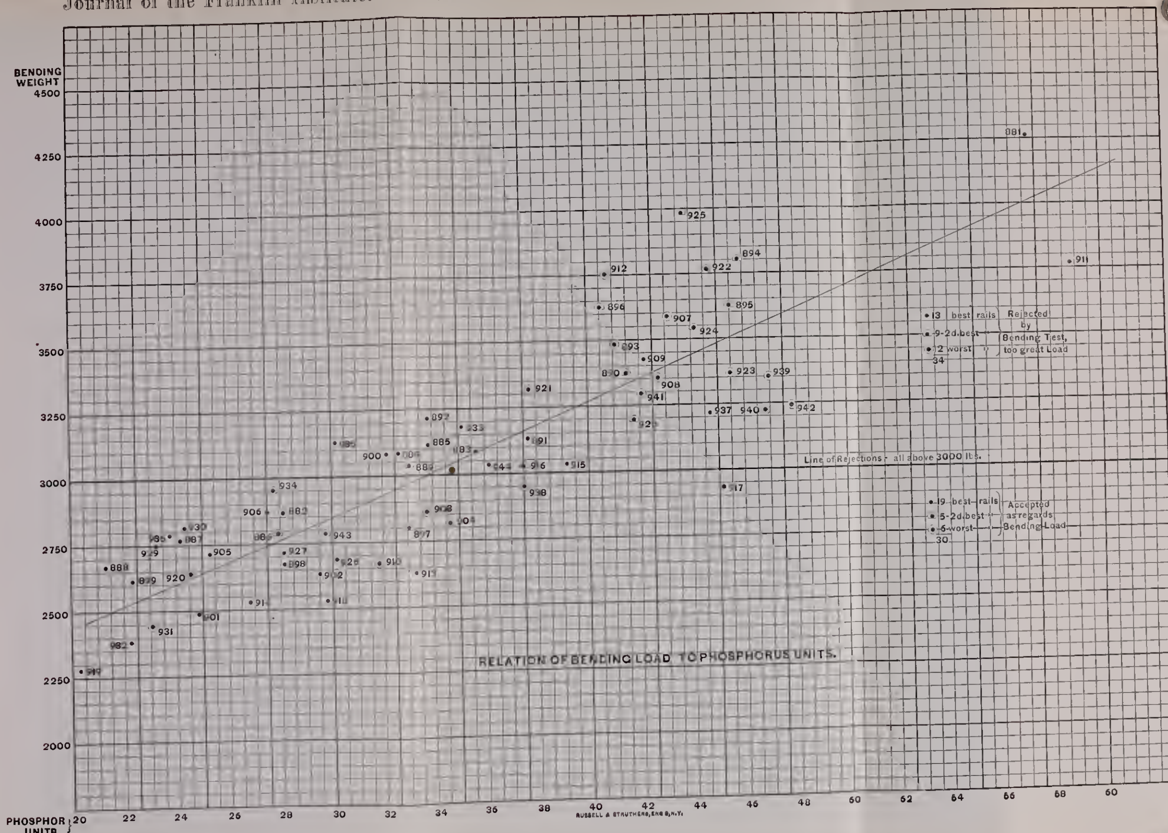
If, in any large series of observations of the values of different variable quantities, such as are here dealt with, there exists any law of interdependence of such variables, the figures representing such observations can be plotted on cross-section paper, and the law will be plainly revealed by a curve or straight line drawn through the various observations. I have thus plotted the results of Dr. Dudley, and have made 42 curves, or attempts at curves, to discover, if possible, the existence of any law of interdependence of the variable wearing capacity in the six series of rails, viz., tangent grade, curve grade high side, curve grade low side, tangent level, level curve, high side and level curve low side, with the seven other variables; bending weight, deflection, carbon, silicon, phosphorus, manganese and phosphorus units. Here is the diagram (Plate I) with the 42 attempts to form as many curves. You will see there is neither curve nor straight line here—nothing but a heterogeneous mass of ups and downs and straights across—not the slightest indication of any law in any single curve which is not contradicted by another curve of the same variable in another series.

This set of curves, or rather zigzags, shows plainly my reason for separating Dr. Dudley's 32 bad rails into two series, one of 18 worst rails, the other of 14 which nearly approached the good rails. In the

curves of the tangent grade rails the two worst rails, 882 and 893, in their plotted positions in the curve remove themselves far from the main body of these rails, and therefore in contrast with the others they are justly named the very, *very* bad rails of this lot. This grouping of the very bad rails far away from the better rails is still more plainly seen in the level tangent rails, where 13 rails are comparatively near together in position, and the other three are far removed from them, thus plainly indicating that if we wish to consider the relation of the chemical composition of these 16 rails to their wearing capacity, the method of averaging the 8 faster-wearing and the 8 slower-wearing—which Dr. Dudley seems to think is the only good one—is not the best by any means, but that to obtain an intelligent deduction the 13 fairly good rails must be contrasted with the three rails which are widely different from them in wearing capacity.

I have said that these zigzags indicate the absence of any law of relation between wearing capacity and the six variable quantities under consideration. I am surprised that Dr. Dudley did not include his phosphorus units in his specification, as I regard this idea of phosphorus units as a most valuable conception, and one likely to lead to the advancement of our knowledge of the relation of the physical to the chemical qualities of steel. For this reason I have included phosphorus units in my plate of zigzags, although Dr. Dudley has not named it in his specifications. I thought it probable that by plotting the phosphorus units, I might possibly discover whether they had any relation to wearing capacity, although carbon, phosphorus, silicon, and manganese had no such relation. I discovered none. But there is a very marked peculiarity in the zigzags representing phosphorus units besides that of general relation to the four chemical elements which follow as a matter of necessity, namely, the almost entire parallelism of the lines of phosphorus units and bending weight. This suggests at once the existence of a relation between phosphorus units and bending weight, and I have, therefore, made another diagram, plotting the figures expressing this relation. The accompanying Plate II shows bending weight at the side of the section paper, and phosphorus units at the bottom. You will plainly notice that there is an evident trend of the dots representing the rails in the direction of an ascending inclined straight line. The whole range of bending weights being 2000 pounds, the deviation of the position of any rail from this inclined straight line (with only two exceptions) is an ordinate representing less


892
93:
885
883
389
903
904
97
913
34


BENDING WEIGHT
4500
4250
4000
3750
3500
3250
3000
2750
2500
2250
2000
PHOSPHOR UNITS
20
22
24
26
28
30
32
34
36
38
40
42
44
46
48
13 best rails
9-2d.best
2 worst
34
Rejected by Bending Test, too great Load
Line of Rejections - all above 3000 lbs.
19 best rails
5-2d.best
6 worst
30
Accepted as regards Bending Load
RELATION OF BENDING LOAD TO PHOSPHORUS UNITS.
RUSSELL & STRUTHERS, ENG'S, N.Y.

than 150 pounds. The inclined straight line is the average direction of trend of these dots, and the individual dots do not deviate greatly from this average direction. The law of the relation between phosphorus units and bending weight is thus clearly established, and it is this: *The increase of bending weight is directly proportional to the increase of phosphorus units.* The discovery of the law is a strong presumptive proof of the correctness of Dr. Dudley's hypothesis in regard to the hardening influence of the four chemical elements upon steel, viz., that the relations of phosphorus, silicon, carbon and manganese are to each other in respect to hardening as the numbers 1, $\frac{1}{2}$, $\frac{1}{4}$ and $\frac{1}{5}$. Dr. Dudley is certainly to be congratulated upon this discovery of presumptive proof of his hypothesis.

The fact of the law of relation of bending weight to phosphorus units being so plainly indicated in these 64 analyses and tests, while there is no such indication from these tests of any similar relation existing between wearing capacity and phosphorus units, or between wearing capacity and carbon, phosphorus, silicon, manganese, bending weight or deflection, is almost absolute proof in itself that such relation does not exist at all, or, as stated in the first part of my remarks, if it does exist it is entirely obscured by the influence of other variable quantities not considered in Dr. Dudley's paper.

On the diagram on which is plotted the relation of phosphorus units to bending weight I have indicated the position of each of Dr. Dudley's 32 slower-wearing rails by a single dot. The 18 worst rails of the 64 are indicated by a dot surrounded by a circle, and the remaining 14, called second-best on the diagram, are indicated by a dot surrounded by a semicircle. By drawing the line between rails which would be rejected on Dr. Dudley's specification for bending weight alone, viz., not above 3000 pounds, and counting those above the line, it is seen that there are 34 rails rejected out of the 64, and of these 34, 13 are the best rails of the series (Dr. Dudley's slower-wearing rails), 9 are the second-best rails,—almost as good as the former,—and 12 are from the 18 worst rails. Counting the accepted rails, a total of 30, 19 are best rails, 5 are second-best, and 6 worst. As already shown, of these 30 rails, which might be accepted on the ground of their bending weight being below 3000 pounds, only three of them could pass the gauntlet of all the other five specifications, and one of these three is a second-best rail.

I think I have now proved my first proposition, but I must antici-

pate an objection which Dr. Dudley may possibly raise. He may say that my argument against his formula, based upon the rejection of 61 rails out of 64, is invalid, because these rails were not made according to his formula. Let them be made according to my formula, he may say, and they will not be rejected, and they will give good service. I regret that he has given us the record of only three rails which do conform to his formula; they are not sufficient in number to draw valid conclusions from, but, such as they are, here is the conclusion they lead to. Two of them are slower-wearing, one a faster-wearing rail; therefore, out of three rails which conform to the formula, the chances are that one rail will not be a good one. Are the railroad companies satisfied with such a result as this? But suppose, for the purpose of admitting a greater number of rails into our accepted list, we relax the specifications slightly. We will say that rails must be made according to Dr. Dudley's formula, but if a rail happens to conform to five of the six specifications and fails in only one, and that one manganese, the least injurious element considered (Dr. Dudley himself considering it only one-fifth as bad as phosphorus), then we will not reject that rail. Our list of accepted rails will then read as follows:

No.	Service.	Order in Dr. D.'s table.	Max. load.	Deflection.	C.	P.	Si.	Mn.	P. units.
920	Level tangent,	10th in 16	2630	190°	·293	·063	·039	·326	24·6
932	" curve, low side,	1st " 8	2340	190°	·269	·047	·026	·372	22·4
943	" " "	6th " 8	2780	159½°	·314	·061	·025	·602	29·8
931	" " high side,	5th " 8	2430	190°	·260	·047	·029	·416	23·1
887	Tangent grade,	1st " 16	2770	190°	·287	·048	·023	·435	24·2
886	" "	13th " 16	2790	190°	·349	·069	·026	·404	27·9
899	Curve gr., low side,	1st " 8	2620	190°	·263	·051	·038	·326	22·3
910	" " "	5th " 8	2660	190°	·343	·098	·020	·478	31·8

Here are eight rails, unquestionably the softest rails of the series, conforming exactly to Dr. Dudley's specifications, except four which have manganese less than one-tenth of a per cent. higher, and one with manganese two-tenths of a per cent. higher than the specifications. We ought to expect that all or nearly all of these eight rails would be included in the thirty-two rails designated by Dr. Dudley as slower-wearing, but, on the contrary, there are only three of them so included, and five of them are the faster-wearing rails. It has been said that exceptions prove the rule, but there is such a thing as having more exceptions than rule, and I think we have such a case before us.

After Dr. Dudley has spent some years investigating the difficult problem of determining what physical or chemical properties have an influence upon wear, he tabulates his results; he studies them carefully; he then proceeds to write his report and to draw his conclusions. In the report we find the following words:

"Giving our attention now to the tables, I think the first observation will be that there is no absolute gradation in physical qualities, or in chemical composition, applying to every rail in each group, which corresponds to the gradation in amount of metal lost per million tons."

Then he tells us we ought not to expect such uniformity, for two reasons:

1st. Errors in determining loss of metal and tonnage.

2d. I quote his own words: "I am not aware that it is known as yet exactly what wear is, or what it is dependent upon; . . . . . whether wear is a direct function of the tensile strength of steel, or of its elongation, or of its elastic limit, or of its resilience, or any combination of these, or indeed, as seems somewhat probable, of the amount of distortion by bending that a piece of steel will suffer, is a problem yet to be solved."

It certainly is a problem *yet to be solved*, and it will take many years to solve it. Dr. Dudley should have stopped here, and drawn the conclusion which I have drawn, namely, that, within the wide range of analyses and bending tests which I mentioned in the first part of these remarks, as far as these sixty-four tests show anything, they indicate that whatever wear is or may be dependent upon it is not dependent upon carbon, phosphorus, silicon, manganese, bending weight and deflection. Instead of this, in one breath he admits he does not know what wear is dependent upon, and in the next he formulates the extraordinary *non sequitur* that it is dependent upon carbon, phosphorus, silicon, manganese, bending weight and deflection, and recommends that the Pennsylvania Railroad Company demand that rails be made on specifications, based on these six variables, so narrow that to fill them would cause the constant rejection of enormous quantities of steel, and a consequent enhancement of the price of rails, probably ten or twenty per cent., without any certainty that such rails would be any better than those the steel-mills are now making.

I earnestly hope that the investigation which Dr. Dudley has so ably carried on will be continued. I hope the Pennsylvania Railroad

Company, or preferably a combination of several railroads, will institute the prolonged investigation which I think will be necessary to solve this deep problem; that they will take a hundred or more rails, watching and noting down carefully every detail of their manufacture as well as their analysis; that they will be carefully weighed before, during, and after service; that their crop-ends will be tested before service, and the rails themselves after removal; that all the sources of error which Dr. Dudley admits in the present investigation may be removed, and that enough facts may be gathered and tabulated so that the conclusions which may be drawn from them may be apparent to every one without labored discussion or heated argument. But I venture to prophesy that after this investigation shall have been completed, and a formula adopted which shall be satisfactory to both manufacturers and consumers, that formula will not be the one now under discussion.

I hope Dr. Dudley will pardon me if I have been unduly severe in my criticism, and consider that I have written my remarks hastily and at a time which should have been given to needed rest. I differ with him only as regards the conclusions which he has drawn. I appreciate the value of his labors, and only make public my own conclusions in the hope of contributing to the advancement of the science of steel-making, in which we are all so deeply interested.

Dr. August Wendel, Troy, N. Y.: Dr. Dudley's last paper gives, certainly, very valuable and interesting information regarding the wear of rails under different conditions. His results concerning the composition of rails, explode, rather startlingly, some old theories regarding the wear of rails, and I think after his formula is simplified it will be *one* good formula to work by.

As he now arrives at the same conclusions reached in his first paper, some of my remarks will apply to both, although I would not attempt to add anything directly to the distinguished criticism the first paper received.

Regarding his silicon percentages, I must say I cannot see any reason why they have not been disregarded entirely within the limits of his investigation. In following Dr. Dudley's reasoning in the use of averages and applying inductive methods, I cannot see what importance can be attached to this element, since, in his first investigations, the good and bad rails averaged nearly alike, and in his last series the

rails which wore best, showed even more silicon than the bad ones. I regard, therefore, the silicon an inconsiderable factor in making out the phosphorus units, without considering here the actual influence of this element on hardness,—an influence which is greatly overestimated in the formula.

With regard to the effect of manganese, I cannot agree with his conclusions. In ordinary working with full-blown steel, manganese has more or less the function of carbon, provided the spiegel is constant, and consequently should not be introduced into a formula for daily working as an independent coefficient.

In March, 1875, I made a report to my employers concerning the unsatisfactory working of steel in blooming.* I then came to the conclusion that steel ingots, in order to roll well, ought to contain:

$$Mn = 0{\cdot}8\ (C + \tfrac{1}{2}\ Si) - 4\ P,$$

these symbols standing for the respective percentages of the elements. I maintain to-day, that for good results in blooming, this percentage of manganese ought to be aimed at for rail steel. For this reason I would sooner undertake to make steel according to Dr. Dudley's original formula, viz.:

Carbon,	0·334
Silicon,	0·060
Phosphorus,	0·077
Manganese,	0·491

than according to the one in which he tries to make concessions to the manufacturers, viz.:

Carbon,	0·30
Silicon,	0·04
Phosphorus,	0·10
Manganese,	0·35

Now in spite of all the progress in steel manufacture, we have not succeeded in making up steel by prescription, and what would therefore become of the ingots in which manganese for some cause or other should happen to fall below Dr. Dudley's meagre allowance? The answer will be: "Works must return to the old practice of hammering ingots," but I doubt very much whether even by hammering a sounder bloom, and as a consequence, a better rail is obtained.

German government officials who as a rule are not happy unless

* Transactions, Vol. IV, page 364.

they can make things unpleasant for somebody, must have got hold of Dr. Dudley's formula, as they lately insist upon steel being hammered. Now, there is not the least doubt that some of the steel under Dr. Dudley's investigation was hammered, but I do not deem it necessary to resume that practice, if a larger manganese percentage is used.

I am greatly encouraged in this statement by the analysis of the *rara avis* of the series. I refer to the one showing 0·48 silicon. I would consider it sound metal, since it satisfies my equation regarding manganese, and still, in spite of its increased hardness and its enormous phosphorus units, is satisfactory in wearing capacity.

The conclusions of Dr. Dudley's ingenious experiments seem more simple than he cares to admit, and could be condensed by simply saying: "Use iron low in phosphorus, and do not make the steel too hard."

Regarding tests of steel made from such iron, I would even be more stringent than Mr. Sandberg. Of each blow I would make a bar about one inch square, and bend it cold. It should not be so hard as to resist bending to the shape of a horseshoe, nor should it be so soft as to bend 180 degrees without showing signs of fracture. There would thus be obtained a quality of steel that would more than satisfy Dr. Dudley's pretty theory of infinitesimal teeth by creating those whose tendency would be to neither flatten nor to bend. I think that in making this test, and supplementing it by the carbon test, manganese and silicon would regulate themselves much more nicely than any specification could effect. Every steel maker knows that, should the silicon run high, the heat is blown too short, carbon will be increased considerably, and the test will not stand. On the other hand, if manganese is high, the heat has received too much spiegel (and that is simple awkwardness) and carbon would show the same result as above.

In conclusion, I should think that railroad authorities, under all circumstances, would prefer the steel with which they are now familiar, to a specimen that Mr. Sandberg has described as having broken into seventeen pieces under the wheels. After blowing such low manganese steel, it may be coaxed into a rail, and it is a wonder that it holds together so long as it does, with so great a number of minute flaws.

I would not in any way depreciate chemistry, but I think it should be kept in its proper sphere. Let the chemist look after the quality of pig metal, and apply common sense in the avoiding of extremes; then the most fastidious railroad cannot find fault with the result.

PROFESSOR EGLESTON, New York City: It is not my place as an engineer to apologize for the chemists, but as no one seems disposed to do so, and as they have had more than their share of criticism, I am glad to say that I believe there are chemists in this Institute whose work and word is just as reliable, and perhaps even more so, than that of the average engineer. But we ought to make a distinction; there are chemists and chemists. With the ordinary commercial chemist, who looks upon the science as so much merchandise, I have not a particle of sympathy; but with the chemist who looks upon his profession as engineers do upon theirs, I have every sympathy. When manufacturers and engineers go to a chemist, and ask him to make an investigation, and screw him down to the lowest point, turning the equivalent of his brains into cents and mills, they usually get an exact equivalent in poor work for miserable pay, and no one has or should have any sympathy with them, and the manufacturer under these conditions has no right to ask for any, and no reason to criticise any work that he may get under such circumstances. But I am disposed to think that the chemists who have been represented and discussed at this meeting do their work conscientiously, and that it is as reliable as that of almost any profession.

I believe, however, that this problem of steel rails is being investigated in the wrong direction. I said so at the Pittsburgh meeting, and I think the discussion of this meeting will prove it to all those who have heard it. I think the chemist is incapable of solving this problem unless he goes very far into the domain of physical chemistry, so far that it becomes physics and not chemistry; and that the physicist will be the one on whom we must rely in the future for the elucidation of the subject. The chemist may aid the physicist, but it is my decided opinion that we are looking in the wrong place to get the explanation of the phenomena.

Attention was called at the St. Louis, and afterwards at the Baltimore meeting, to the fact that the pounding which a rail receives from the falling of the engine from a high rail to a low one was sufficient in many cases to account for effects which have not been explained in this discussion. This statement was made after an extended observation had been made of rails laid over many hundred miles of railway in Europe. But if it is true, as Mr. Cloud stated in his papers read yesterday, that the blow of the engine is repeated not only at the end of the rail, but every time the driving wheel makes a revolution, it

will explain much of the giving-way of the rails over their whole length, and the effects of these blows on the physical condition of the steel should be very carefully investigated. It was shown in the discussion of the law of fatigue and refreshment of metals at the meeting in Montreal, that every blow was accompanied by a physical effect which could be rendered distinctly visible. The blow and pressure of the gag which always leaves its trace on the rail is certainly less of a physical effect than the constant and rapidly repeated series of blows which the rail receives from the continual passing of trains, yet the gagging always injures a rail and sometimes destroys it.

At the Baltimore meeting two rails were shown which had been placed in essentially different conditions, and which had been subject to a cold flow of the metal in every part of the rail even to the very outside of the foot. Mr. Metcalf yesterday showed this same kind of a flow in rolls. He also spoke of the ignoring of the copper in the analyses of steel rails. At the Washington meeting in 1876, the statement was challenged that good steel rails had been made containing any large percentage of copper, and though repeatedly promised the analyses of such rails, they have never been produced. Some years ago, in visiting one of the largest steel works where the ingots are compressed, I noticed a jet of blue flame passing out from the bottom of the ingot mould, which I at first thought was phosphorus, but which I afterwards determined to be copper. Certainly, if there is enough copper in the rail to allow of its becoming visible in the color of the flame, there must be enough to influence its physical condition and its life, and we cannot afford to neglect it.

I have also had occasion to show that bubbles produced in the steel ingots were never rolled out of them, and that if they were once in the ingot they remain in the rail and arrange themselves in such conditions that they were sure sooner or later to engender a weakness in the direction which the cavities take. As these bubbles are never rolled out as the rail wears, they come to the surface, and the rail which shows a chemical analysis which is perfect, becomes imperfect from a physical defect. Every one who is familiar with the breaking of metals in a testing machine has been made aware of the fact that these bubbles will often cause metals whose analysis is faultless, to break with a very low tensile strength, while a piece without bubbles, taken from the same sample, will break with a high one. These bubbles are sometimes finer than pin-holes. I have, in investigating this fact in

railroad material, occasionally come across iron and steel which were so fatigued at the time that they left the rolls, that they were really unfit for service; and I have seen new rails which I should not feel justified in placing in any other position than on a side track. This question of fatigue in the course of manufacture has not been discussed at all so far as I know. I took occasion to show in the St. Louis meeting, that a rail taken from the Northern Railway of France, which I had the pleasure of examining, and which was condemned as good for nothing, and which the manufacturers were obliged to replace, when brought to Paris and submitted to chemical and physical tests, proved to be as good as any rails which were in the service. These kinds of physical defects are the ones which are to be looked after; and, in fact, nearly all the defects for which a chemical solution is demanded are physical. I have within the last year had occasion to examine metals and alloys which are fatigued, and find that while chemically the same, they act physically so entirely differently that I hope at some future meeting of the Institute to bring the matter before you.

I am at a loss to know why certain chemical substances which we know are in the iron or are likely to be there, should not be determined, and their effects discussed. Copper is one of these, sulphur is another, titanium and vanadium are known to be sometimes there. Why should not the effects of these substances be discussed?

Mr. Metcalf has alluded to the question of occluded gases. I have had occasion to show recently in my laboratory that as a general thing metals and alloys which were brittle from fatigue, contained a much larger amount of gas than the same metal which was not fatigued. I have also ascertained that the metals in this condition go into solution in a manner quite different from those not fatigued. I think also that the place from which the sample is taken from the rail will make some difference with regard to the results. Some one, I do not remember who, made an investigation some years ago upon the effect of having the rails always rolled in the same direction, and also of having been rolled backwards and forwards, and showed that under the latter course there were of necessity weak spots somewhere near the centre of the rail, yet in all this discussion the methods of rolling have been passed by almost in silence.

Mr. Sandberg in his paper mentions the idea of using a registering dynamometer attached to the punching machines, and of determining the quality of the metal by the resistance which is shown. I think the

first idea of this kind was published by Professor Langley, then of Pittsburgh, who, while making some investigations for Messrs. Miller, Metcalf and Parkin, announced as the result of a series of dynamometric experiments that abrasive resistance was the term which should be used in regard to steels of different wearing qualities. I have had extremely delicate dynamometers attached to the instruments of precision with which I am making the investigation on the fatigue of metals, and hope soon to communicate the results of the investigation made with them to the Institute.

I wish again to call attention to the fact that we are using the words "hardener" and "hardness" without any real idea of the meaning of these words. When we say hard and soft, as we have been constantly doing during this discussion, is it quite sure that every one has exactly the same meaning in his mind? Certainly, when hard is used in distinction from soft, we mean not the capacity of wear, but the capacity of resistance to penetration, to fracture, or some other resistance, and do not always mean the capacity of resistance to abrasion or crushing, which the discussion would sometimes seem to imply were the only qualities requisite to constitute a good rail.

J. W. Cloud, Altoona, Pa.: I would call the attention of the Institute to the title of Dr. Dudley's interesting and valuable paper,—"The Wearing Capacity of Steel Rails in relation to their Chemical Composition and Physical Properties." Here are two separate and distinct questions: 1st, The wearing capacity of steel rails in relation to their chemical content; and, 2d, the wearing capacity of steel rails in relation to their physical properties.

The discussion has been almost exclusively confined to the former question, on which there may be many differences of opinion in matters of detail without greatly affecting the result. The latter question has been ignored, and I wish to call attention to it, particularly, because it is the practical question after all from the consumer's standpoint, and because the paper under discussion is more decisive on this question than on the other. In fact, I think we must admit that Dr. Dudley has established his main point, viz, that the softer steel, physically, gives the slower wear, contrary to general belief among engineers in this country.

Of course, we are all interested in the whole subject from a scientific standpoint, but as the discussion has been participated in largely by

manufacturers of steel, with a business animus, and as they have endeavored to overthrow Dr. Dudley's chemical recommendations because they are necessarily the most vulnerable points in the paper, I wish to recall attention to the facts of the physical properties as being the consumer's only practical guide, and as affording the most conclusive results in the data before us.

The consumer of steel rails cannot test every blow chemically, but he can do so physically, and it is my opinion that a bending test of a whole rail section, say three feet long, under steady pressure, instead of a drop, with a specified deflection, without cracking, and a load not to be exceeded to produce this deflection for each rail section, will be a good practical test, not burdensome to manufacturers, and one that will insure consumers such a degree of softness in steel as they may desire.

They should not attempt to dictate to manufacturers how this degree of softness shall be obtained chemically, but allow them full freedom to do as they please, so the proper physical properties are had. Further, we are now in possession of the information requisite to make such specifications with the certainty of greater safety as well as more economical results in the life of rails.

I have recently had opportunities at Altoona to see other and very convincing evidences of wear, as compared with physical softness. The locomotive driving-wheels probably cause at least one-half of the wear of rails, and the forces which go to wear the rails from the tires, must react with similar intensities on the tires themselves. We therefore have in driving-wheel tires an opportunity to see the wear in a more concentrated form, so to speak, or with rates as well as differences magnified. I have recently found differences of one inch to two inches in circumference of two tires on the same axle when coming to shop for turning, and it is invariably evident that the smaller tire is much the harder, the chips from it being short and brittle, while chips from the larger tire are much longer and tougher. In the worst case I have observed, viz., two inches difference in circumference, this difference in hardness, as observed from the cutting, was more marked than in the other cases. Tires are always grouped in sets by the manufacturers from their knowledge of the chemical composition of the steel, with an attempt to get those in one set which have the same degree of hardness, so that the wear shall be equal all around; they succeed pretty well on an average, but I have been noting the exceptions.

Jacob Reese, Pittsburgh, Pa.: I have been very much interested in the reading and discussion of Dr. Dudley's paper. As far as it relates to the data of work performed by the rails, and the determination of their physical and chemical properties, I have nothing but commendation of Dr. Dudley to express, as the investigation covered a greater range, and was performed with more care in detail, than any similar work which has come under my notice. But I beg leave to differ with Dr. Dudley in his conclusions.

What are the factors of hardness? Are they not carbon, silicon, phosphorus, and manganese? Now it is an undisputed fact among metallurgical experts that pure carbon and pure iron make the best steel of all degrees of carburization, and for all purposes. While carbon hardens, it also strengthens the metal, but silicon, phosphorus, and manganese, in hardening, make the metal also brittle, and are injurious in any amount. Carbon should be called a *strengthener;* and I claim that a steel rail made hard with carbon, with the other three hardeners absent or reduced to a minimum, will carry a greater tonnage than any of Dr. Dudley's soft rails.

But until the basic process is put into operation in this country we cannot expect to produce Bessemer or open-hearth steel without the presence of silicon, phosphorus and manganese, in considerable quantities, and I greatly doubt the possibility of reducing the percentage of any of them by the present practice without seriously diminishing the output, and correspondingly increasing the net cost of production; which is an important question, since the increased life of the rail may be more than balanced by its increased cost.

I think that the soft rails performed a greater amount of work, because they contained a less amount of silicon, phosphorus and manganese (*brittlers*, if I may so term them), and that carbon does not reduce the wearing capacity of rails. I believe that a rail made by the basic process, with silicon, phosphorus and manganese reduced to a minimum, and containing 0·60 carbon, will be stronger and tougher, and will carry double the tonnage of any of Dr. Dudley's soft rails.

(To be continued.)

Pronunciation of Foreign Languages.—The Polyglot institute at Paris proposes to try a phonograph of new construction, in order to teach its pupils how to pronounce correctly the difficult words of foreign languages.—*Chron. Indust.* C.

BOILER EXPLOSION AT GAFFNEY & CO.'S DYE WORKS, PHILADELPHIA, JUNE 1, 1881.

By W. Barnet Le Van.

A cast iron flat boiler head only two inches thick would not be considered by an experienced engineer strong enough for a cylinder boiler thirty-six inches in diameter, especially when the rim by which it was secured to the shell plate was only 1·625 ($1\frac{5}{8}$) inches thick, with scarcely any fillet at its junction, and this rim being placed so as to project into the shell of the boiler, when it should have projected outside, according to the usual practice.

The insufficiency of such a boiler head was proved, with fatal results, on the 1st of this month, at the dye works of the above-named firm. The head of this boiler blew out, killing three persons and wounding eight, besides damaging property to the amount of about $15,000.

The boiler, which was new, was built by Messrs. Sidebottom & Powell, of Frankford, Philadelphia, for Gaffney & Co., and was inspected by a Steam Boiler Insurance and Inspection Company on the 2d day of March, 1881, and tested in connection with two similar ones on the 7th of the same month, by hydraulic pressure of ninety-five pounds per square inch, and was passed by this Insurance and Inspection Company as capable of carrying a working steam pressure of sixty-five pounds per square inch above the atmosphere.

During the examination of this company's inspector, before the Coroner's Jury, he stated, in answer to a question from one of the jurymen, that "there were quite a number of boilers in this city which had cast iron heads, and that his experience had taught him that a concave cylinder boiler head was stronger than a flat head." Yet he passed this flat cast iron head without any comment at the time to the attendant owners or the insurance company in whose charge it was placed.

This disc of cast iron (the boiler head) took its full load of about 30 tons without any other support than that at the connection of the rim with the shell of the boiler.

The cylinder head of a sixteen inch diameter engine would have

been made one inch thick, and yet this boiler head, having twenty times the area, upon which the pressure was exerted through a threefold greater leverage, was but twice as thick. A thirty-six inch engine cylinder head would have been cast hollow concave, or else stoutly ribbed on its face; yet this boiler head was a plain flat disc, only two inches thick, and the rim, which is at right angles to the head, only 1·625 ($1\frac{5}{8}$) inches thick, with scarcely any fillet at its junction, and this rim being placed inside of the boiler the steam pressure acted on it by tension, in place of by compression, at its weakest point. Had the pressure acted by compression on the rim of the head, it would have resisted nearly twenty per cent. more pressure before parting. An examination of this head after the explosion shows that it was under internal strains resulting from bad design as to form and proportion.

It is a well-known fact in mechanics that, in casting plates of iron, if one part is less in thickness than the larger portion of the casting, especially when two surfaces join each other at right angles, as was the case with this boiler head, the difference in cooling will cause internal strains, in some cases to such an extent that when a casting becomes cool it may break by the unequal contraction of the several parts, without any pressure whatever. The effects of these strains to reduce the ultimate strength of the boiler should be taken into account as an important element, as in this case a part of the factor of safety was already expended, by reason of the improper construction of the boiler head in this respect.

Another great defect in this boiler head was that the man-hole plate was not properly fitted to its seat on the head; the seat was a planed surface, and the man-hole plate was a wrought casting, so that in making a steam-tight joint between the two a strain was induced additional to that due to the improper design and proportions of the head, as before explained. Had this boiler head been made concave, and of an average thickness of 1·25 ($1\frac{1}{4}$) inches, with the convex side inward, with a round man-hole in place of an oval one, and thickened around the same on both sides, I have no doubt whatever it would never have given away; but, better still, it should have been made of wrought iron, dished, and with the convex side projecting from the boiler.

Now that a board of engineers is about to revise the boiler inspection laws of this city, would it not be well to add a clause providing that cast iron should be dispensed with altogether in the construction

of steam boilers, unless so employed as to conform with well-known and approved design and proportions, and so placed in the boiler as to resist the greatest amount of steam pressure with the least amount of metal?

The writer does not hold cast iron to be an improper material, but from the fact that its proper form for resisting heavy pressures is not understood by the majority of our boiler makers, and even with the most skillful moulders a perfectly homogeneous casting is the exception and not the rule. The thinner a casting is made the more homogeneous it will be, all things being equal. Assuming the form and design to be unexceptionable, the use of cast iron is attended with greater risk than that of wrought iron, and should be regulated by such judicious proceedings as will tend, as far as possible, to render its employment (which is often convenient and desirable) sufficiently safe to be unobjectionable upon the grounds above referred to.

Philadelphia, June 8th, 1881.

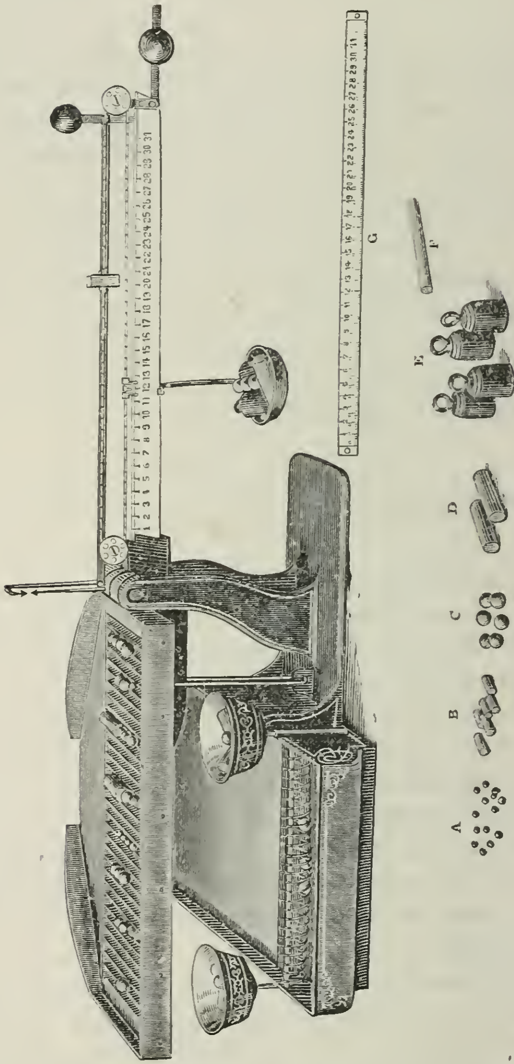
AUCHINCLOSS' AVERAGING MACHINE.

The Averaging Machine (or Book-keeper's Assistant) gives mechanical expression to an important branch of mathematical science, and employs manual instead of brain work in the process of determining the average date on which payment falls due.

It seems odd to think of so imponderable an element as an *average date* being determined by material objects, such as leaden bails, or bits of metal, and being weighed out like articles of merchandise. This, however is not only practicable, but the apparatus will determine with unfailing accuracy the average dates of more than one hundred accounts per hour. It requires no skilled mathematician for its manipulation, as the same results can be secured by those who know little about the use of figures.

The machine is exceedingly simple. It consists of a grooved platform, balanced by a scale beam and pan. The pan is so coupled with a moving counterweight that the equilibrium of the system is preserved for all positions of the pan. The weights bear to each other the ratios of 1 to 10 and of 1 to 100, so that they may be used to represent units, tens, hundreds; tens, hundreds, thousands; hundreds, thousands, tens of thousands, and so on.

The grooves of the platform represent the days of the month, and the notches on the scale beam the average dates. In solving any



AUCHINCLOSS' AVERAGING MACHINE.

problem, the weights are first distributed over the platform so as to represent the several purchases made on the respective days. The scale pan is then laden with an amount *exactly* equal in weight to the entire load upon the platform. After the machine has been thus

laden the scale pan should be moved along the scale beam until it reaches a point where the weights are in equilibrio. The reading of the scale beam gives the average date of the purchases.

The entire process requires so little thought that one can determine a large number of averages without fatigue, and can verify the work by simply glancing a second time over the platform and pan for re-assurance as to the proper location of the weights. After the solution of any problem the weights are dumped and screened, thus preparing the machine for the next solution.

It is curious to note that the same machine can be used for solving a great variety in simple and inverse proportion, besides many others of a more intricate character, all of which may be determined with the greatest rapidity.

RADIO-DYNAMICS. II.

By Pliny Earle Chase, LL.D.

Abstract of Lectures delivered before the Franklin Institute, March 10 and 17, 1881

Continued from page 65.

We have confined ourselves thus far to the simple dynamic laws of radial action to or from centres of energy and of inertia. I hope to satisfy you that those laws are sufficient for the explanation of all varieties of physical phenomena, and that radio-dynamics is, therefore, the foundation of all dynamics, the UNIVERSAL physical science, of which photo-dynamics, thermo-dynamics, electro-dynamics, cosmo-dynamics, moleculo-dynamics and chemistry are branches.

Following the Baconian method, I will first lay before you some general principles, followed by some additional FACTS to which I have been led by applying mathematical principles to the study of radio-dynamics, and then interpret those facts, in the hope of making them helpful towards the strengthening of hypotheses and the discovering of new facts or laws.

The elements of physical energy are mass, m, and velocity, v. The total energy of any force is called its *vis viva*, or living force. It is measured by the *work* that it is able to accomplish against uniform resistance, and is represented by $\frac{mv^2}{2}$.

La Place defined velocity as "the ratio of the space to the time

employed in describing it." In recent dynamical treatises the space traversed, or length, is represented by l; time of traverse, by t; velocity, by $\frac{l}{t}$; energy, by $\frac{m\,l^2}{t^2}$.

The greatest or controlling energy must, of course, be represented by the product of the greatest mass by the square of the greatest velocity.

The greatest mass of which we have any practical knowledge is the mass of the Sun, M_0; the greatest centripetal acceleration is the force of gravity at Sun's surface, G_0; the greatest velocity of wave propagation is the velocity of light, or the velocity of radiation, V_0, at the seat of greatest centripetal acceleration.

Action and reaction being equal and in opposite directions, we may reasonably look for some simple relation between the centripetal and centrifugal maxima, G_0 and V_0.

If we regard Sun's radiating energy as an action, its reaction must be dependent upon its inertia, or mass.

The velocity of projection against uniform gravitating resistance is represented by gt, t being one-half the time of flight. The height of projection is $\frac{gt^2}{2}$. The height of projection which would give the velocity of wave propagation in an elastic medium is one-half the "height of a homogeneous atmosphere" of the medium.

Every particle of the Sun's surface is continually solicited by gravitating tendencies, G_0 and G_u, towards its own centre and towards the centre of the universe. In each solar rotation the particles are alternately projected from and drawn towards the centre of the universe, C_u.

We may, therefore, let T_0, $= \frac{1}{2}$ solar rotation, represent the time of cyclical equality of action and reaction between solar inertia and universal inertia, or between solar gravitation and æthereal undulation.

Then $L_0 = G_0\,T_0^2$ is the height of a homogeneous æthereal atmosphere, at Sun's surface, which would have a velocity of wave propagation equivalent to the velocity of light; $\frac{L_0}{T_0} = V_0$ is the velocity of light.

All forms of energy, mechanical, thermal, photic, electric, magnetic

or chemical, are derived from and can be compared with the maximum energy, $\frac{M_0 L_0^2}{T^2}$.

1. All astronomical, barometric or other mechanical estimates of solar mass and distance involve the proportionality, $g \propto \frac{m}{l^2}$.

2. The arbitrary units of thermo-dynamics are based upon the work done against the centripetal accelerations of superficial terrestrial gravity. The unit of acceleration, or the sum of accelerations in unit of time, at unit of distance, is proportioned to mass. Therefore, if we designate Earth's mass by m_3, we have the proportion

$$M_0 : m_3 :: V_0 : u_3$$

V_0 is the velocity acquired during the cyclical actions and reactions of solar condensation and æthereal elasticity, at Sun's surface; $u_3 =$ ·56558 mile is the velocity acquired during the cyclical actions and reactions of water congelation and vaporization at Earth's surface. Dividing by $\sqrt{180}$ for the Fahrenheit scale, or by $\sqrt{100}$ for the Centigrade scale, we have the arbitrary units of velocity, ·042156 mile for 1° F., ·056558 mile for 1° C. The equation $V = \sqrt{2gh}$ gives $h =$ 772 ft. for 1° F., or 1389·6 ft., = 424 metres, for 1° C.

Combining these heights with the arbitrary units of mass, we have $J, = 772$ ft. lbs., for the English thermal unit, and $C, = 424$ kilogrammetres, for the calorie or French thermal unit.

3. Thermal, mechanical and photo-dynamic energies may be compared with energies of chemical combination, through the ratios

$$M_0 : m_3 :: V_0 : u_3 :: h_0 : h_3$$

Earth's mean distance from Sun, or height of solar projection, is represented by h_0; h_3 is $\frac{1}{4}$ of $\frac{2}{3}$ of the height to which water vapor would be thrown, against the retardation of gravity, by the combining energy of oxygen and hydrogen, the two constituents of water; $\frac{1}{4}$ is the length of a conical pendulum which would vibrate in the same time as a linear pendulum of unit length; $\frac{2}{3}$ is the ratio of *vis viva* of wave propagation to the mean *vis viva* of the oscillating particles which originate the waves.

4. Cosmical, electrical and photo-dynamic energies may be compared by means of the ratio

$$M_0 V_0 r_3 : m_3 V_0^2 :: m_3 V_0^2 : m_3 V_3 r_3$$

Sun, M_0, is at the centre of nucleation in the solar system; Earth,

m_3, at the centre of condensation; Jupiter, m_5, at the nebular centre; v_3 is Earth's aphelion or "nascent" orbital velocity.

5. The electro-static, magnetic and electro-kinetic units of energy can all be derived from the above expression for Earth's photo-dynamic energy, $m_3 V_0^2 = \frac{m_3 L_0^2}{T_0^2}$, through the equations $[e E] = [m \Omega] = [p C] = \frac{M L_0^2}{T_0^2}$.

The bracketed symbols represent, respectively, quantity of electricity; line integral of electromotive force, or electric potential; quantity of free magnetism, or strength of a pole; magnetic potential; electro-kinetic momentum of a circuit; electric current.

6. Atomic energy, or energy of unit volume, can be compared with Earth's photo-dynamic energy of unit volume, $\frac{m_3 L_0^2}{r_3^3 T_0^2} = \frac{m_3}{L_1 T_0^2}$, and with corresponding electric energies, through the equations

$$[D E] = [B H] = [C V] = \frac{M}{L T^2}$$

The bracketed symbols represent, respectively, electric displacement (measured by surface density); electromotive force at a point; magnetic induction; magnetic force; current electric intensity at a point; vector potential of electric current.

7. Electro-chemical and electro-magnetic energies may be compared with thermal, photo-dynamic and other energies through the proportion

$$\varkappa : \mu :: M_0 t_a^2 \times M_0 v_3^2 : m_3 t_n^2 \times m_3 V_0^2$$

I designate Weber's units of electro-chemical and electro-magnetic force by $\varkappa$ and μ, respectively; t_a is the time of acquiring orbital velocity, or incipient associative energy, at Laplace's limit of equal velocities of rotation and revolution; t_n is the time of acquiring nucleal nascent or dissociative velocity. The ratio of t_a to t_n is the same as the ratio of the diameter of a circle to its circumference, $1 : \pi$.

8. Total magnetic force, φ_0, can be compared with the reactions of terrestrial magnetic force, φ_3, by the proportion

$$M_0^2 : \pi^4 m_3^2 :: \varphi_0 : \varphi_3$$

The reactions of orbital tendency are $t_a M_0^{1\frac{1}{2}}$, $t_n m_3^{1\frac{1}{2}}$, respectively. Centripetal undulation varying as the fourth power of orbital velocity, we have the ratio $t_a^4 M_0^2 : t_n^4 m_3^2$, or $M_0^2 : \pi^4 m_3^2$.

9. In the actions and reactions between the centre of nucleation, M_0,

and the centre of condensation, m_3, there are continual tendencies towards the centre of gravity, the centre of lineal oscillation and the centre of conical oscillation, as well as to centripetal and orbital motions. These are all satisfied by the proportion

$$M_0 : m_3 :: (2 \times 3 \times 4)^4 : 1^4 :: 331776 : 1$$

In order to show the closeness of accordance between the theoretical results and those which have been deduced from observation or experiment, I make the following comparisons:

1. The theoretical oscillatory value of Sun's mass, (9), gives 92,785,700 miles for Sun's distance; 8·809″ for Sun's parallax; 25·496 days for Sun's rotation; 299,943 kilometres per second for the velocity of light. These values differ by less than $\frac{1}{10}$ of one per cent. from Faye's, Michelson's and La Place's estimates.

2. The difference between the theoretical and experimental thermodynamic velocities is less than $\frac{3}{4}$ of one per cent. Perhaps, when proper allowances are made for the temperature at which the experiments were performed, the accordance will be found to be exact.

3. The difference in the combining energy of hydrogen and oxygen is less than $\frac{1}{35}$ of one per cent.

4. The difference in Earth's "nascent" velocity is less than $\frac{1}{11}$ of one per cent.

5, 6. The difference which is indicated by the latest electro-dynamic and electro-magnetic investigations is less than $\frac{1}{50}$ of one per cent.

7. Weber's experimental determination of the electro-chemical unit was $\frac{2}{5}$ of one per cent. greater than the theoretical value.

8. The experiments of Joule and others, in Great Britain, upon magnetic action and terrestrial reaction, indicate a difference of about $1\frac{1}{4}$ per cent. in the estimate of Sun's mass, and $\frac{5}{12}$ of one per cent. in the estimate of Sun's distance.

I have been obliged to use many technical terms and equations which are doubtless new to most of you, but you can all see how close is the agreement of the values which are derived mathematically from simple radio-dynamic action, with those which have been obtained experimentally by the most careful observers. You will have noticed how often I have called your attention to evidences of the fourth fundamental law, which is generally known as "Fourier's Theorem," viz.; every periodic vibrating or orbital motion can be regarded as the sum of a certain number of pendulum vibrations.

To illustrate this law I have prepared a stretched cord, to which I

have suspended three balls, at equal intervals, by strings of equal length. I set the middle one swinging, and you see how it gradually imparts its motion to the two lateral balls, and comes to rest itself. Now the side balls are losing their motion, and the middle one takes it up again, until it swings almost as far as at first, and its companions hang motionless. These alternations continue until the resistance of the air overcomes the motion, and the balls are all quiet, as at the beginning. The motion, however, has not ceased; it has merely been imparted to the air, and by the air to the æther, so that we do not see it, and we are unable to follow it through all its changes.

When the regularly recurring vibrations of an elastic medium are transmitted to the ear through a suitable medium, they produce the sensation of sound; if the interval between the successive vibrations is short enough, the sound is musical. We may distinguish between the music in the sounding body and the music which we hear. A bell may ring when swung by the wind, but it will give no sensation of sound unless its vibrations reach the drum of the ear with energy enough to make it vibrate. I strike this tuning fork, but you hear nothing except a single ring. I bring it near the opening of this wooden box and the oscillations of the fork are imparted, first to the air, then to the wood, and the note of the fork is sounded, loud, clear and musical.

It is not necessary that the unison should be exact, or, in other words, that the fork and the resonator should make the same number of pendulum swings in the same time. If the vibrations coincide at intervals which are so short that the ear cannot distinguish between them, one body, for example, making three vibrations and another two in the hundredth part of a second, both sounds are said to be harmonious. Whenever any elastic medium is vibrating, its waves tend to produce harmonic vibrations in all bodies which they strike.

If the elastic force of the æther is more than a million million times as great as that of the air, its musical rhythm, if our ears were sensitive enough to hear it, should be of a far higher order than that of any earthly choir. Chemical atoms and molecules, the particles of heated or electrified bodies, satellites, planets, suns, stars and nebulæ, all intercept the luminous waves. Hence there should be a continual tendency to relative positions which would make all the motions harmonic, or rhythmical. Such a tendency is shown in atomic weights, spectral lines, mechanical equivalents of heat, the various phenomena

of electro-dynamics, the relative positions of satellites, asteroids and planets, and the bonds of union between our own system and the nearest of the fixed stars. The truth of the radio-dynamic hypothesis has been further confirmed by the indication or prediction of harmonic nodes beyond Neptune, as well as between Mercury and the Sun, fourteen of which have been subsequently corroborated by the calculations of various European and American astronomers.

If you throw a ball, or fire a projectile upwards into the air, it will continue to rise until the resistance of the air and the attraction of gravitation have overcome the velocity of projection. If there were no air, and no resistance to the upward flight except gravitation, the relation of projectile velocity to gravitation and height of projection would be represented by the equation

$$v = \sqrt{2gh} \qquad (1)$$

The relation of velocity to time of rise, or to the equal time of fall would be represented by

$$v = gt \qquad (2)$$

In solar rotation each particle is alternately projected from and drawn towards the universal centre of gravity. Substituting the equatorial values of g and t in equation (2), we find that v is *the velocity of light.*

The circular-orbital velocity of a particle at Sun's surface, v_o, is connected with the velocity of rotation, v_r, and the velocity of light, v_λ, by the equation

$$\pi v_o^2 = v_r v_\lambda \qquad (3)$$

The velocity of a particle, at any distance whatever from any attracting centre, can be deduced from equation (3), by means of the eleventh fundamental law, that centripetal accelerations vary directly as the mass and inversely as the square of the distance. Hence we see that *all gravitating velocities are dependent on the velocity of light.*

The distribution and motions of the principal planetary masses are also dependent upon the same velocity. The largest planet is Jupiter, which is more than twice as large as all the other planets. The mean centre of gravity of the solar system is therefore the same as the mean centre of gravity of Sun and Jupiter. The next planet in point of magnitude is Saturn, which is more than two and three-fourths times as large as all the remaining planets, and which is placed at the nebular centre of planetary inertia. When the primitive planetary belt was successively divided into inter-asteroidal and extra-asteroidal belts,

two-planet belts and single-planet belts, the amount of inertia remained unchanged. But in these divisions the rectilinear propagation of luminous waves has been changed into the synchronous oscillations of conical pendulums, introducing the length-ratio of 4 to 1; the synchronous oscillations have been changed into orbital oscillations, in which the time varies as the $\frac{3}{2}$ power of the distance; nebular radii have given place to radii of subsidence-collision, which are only $\frac{2}{3}$ as great; and centripetal tendencies vary as the fourth power of orbital tendencies. If we designate Saturn's mass by m_6, and the mean velocity of rotation of the centre of gravity of the solar system by v_s, these changes and their dependence on the velocity of light are shown by the proportion

$$4^{\frac{3}{2}} M_0 : (\tfrac{2}{3})^4 m_6 :: v_\lambda : v_s$$

Bessel's estimate of the comparative masses of Sun and Saturn was $M_0 = 3501{\cdot}6\ m_6$. Substituting this value in the above proportion, we find

$$v_\lambda = 141815\ v_s$$

The most accurate estimate that we are able to make of v_s is 1·31405 miles per second. This velocity, which is so readily deduced from the velocity of light, is also *the wave-velocity which represents the conversion of water into vapor.*

Let θ represent the latent heat of steam, then well-known laws of thermo-dynamics give us the equations

$$v_s = \sqrt{2gh}$$
$$\theta = \frac{h}{1389{\cdot}6}$$

Solving these equations, we find

$$h = 750{,}098 \text{ feet.}$$
$$\theta = 539{\cdot}794 \text{ C.}$$

Among the various experimental values which have been obtained for θ, the following are generally regarded as the most trustworthy:

Favre and Silbermann,	535·77°
Andrews,	535·90°
Regnault,	536·67°
Tyndall,	537·20°
Despretz,	540·00°
Dulong,	543·00°

According to the kinetic theory of gases the internal movements of the particles of steam are rectilinear. When the steam is condensed, in the form of water or ice, the internal energies tend to maintain a spherical figure. The synchronous oscillations of the two conditions may be represented, according to Fourier's theorem, by linear and conical pendulums. The height to which the vapor would be projected above the water level being represented by 750,098 feet, or 142·064 miles, the length of the conical pendulum is $\frac{1}{3}$ as great, or 47·355 miles, and the length of the linear pendulum is $\frac{4}{3}$ as great, or 189·419 miles. The heat of sphericity should be, therefore, $\frac{1}{3}$ of the latent heat of steam, or 179·93°. Deducting 100° for the expansion of water from the freezing to the boiling point, we have 79·93° for the "latent heat" of ice, or more properly speaking, for the heat which is required to overcome the crystallizing energies of water. The following values have been obtained experimentally:

Desains and De la Provostaye, . .	79·25°
Black, ...	79·44°
Person, ...	80·00°
Hess, ...	80·34°

The combined influence of photo-dynamic *vis viva* and the nebular "subsidence" which was pointed out by Herschel, is shown in the proportion

$$1·01555^2 : (\tfrac{5}{9})^2 :: m_5 : m_6$$

The radius of incipient subsidence for Neptune's orbit (Law 10, p. 61) is 1·01555; $\frac{5}{9}$ is the ratio of the *vis viva* of wave propagation to the mean *vis viva* of oscillating particles (Law 22, p. 62). If we substitute Bessel's estimate for m_6 in the above proportion we find

$$1·03136 : \frac{25}{81} :: \frac{1}{1047·9} : \frac{1}{3501·6}$$

If we take Stockwell's estimate of Neptune's radius of incipient subsidence, 1·0145, we get $M_0 = 1050\ m_5$. Bessel's estimate was 1047·879; Leverrier's, 1050. The difference between the two estimates is only $\frac{1}{5}$ of one per cent. Bessel's is the one which is adopted by the British and American Nautical Almanacs, but the reputation of Stockwell and Leverrier seems to render it probable that the true values may be intermediate between the two which are here given. The photo-dynamic relations of mass and distance become still more striking when we find that the incipient subsidence at the nebular centre, Jupiter, is at a mean proportionate distance between the centre

of condensation, Earth, and the incipient subsidence of the primitive planetary belt, Neptune. This is shown by the proportion

$$1 : 5{\cdot}52 :: 5{\cdot}52 : 30{\cdot}47$$

If we represent Earth's semi-axis major by 1, Jupiter's secular aphelion is 5·52, and Neptune's secular aphelion is 30·47.

These comparisons might be extended almost without end. We have now surveyed the whole field of physical science, and have found, in every direction, that all possible physical energies can be expressed in terms of the greatest and most pervasive energy, through the mass of the Sun and the velocity of light. Simple gravitation, solar rotation, nebular subsidence, orbital revolution and all other gravitating motions, cosmical aggregation, the distribution of planetary masses, the establishment of centres of inertia, condensation and nucleation, evaporation, crystallization, heat, mechanical work, barometric pressure, atomic energy, chemical combination, electricity and magnetism, are all so simply connected by the universal laws of action and reaction in elastic media, that they all furnish ready methods for estimating the mass and distance of the Sun and the velocity of light.

THE PROPERTIES OF AIR RELATING TO VENTILATION AND HEATING.

By Robert Briggs, C.E.

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The surface of the earth is covered by a gaseous body, some forty or fifty miles in depth, which is called the atmosphere. Chemistry has discovered and isolated various gases, some of which, so far as further separation is concerned, may be deemed elementary, while some are chemical compounds of definite proportions of other elementary gases and bodies. In some cases bodies which in their elementary form, at temperatures subsisting in nature, are solid, become portions of chemical combinations as gases at similar temperatures.

The atmosphere is composed mainly of a mixture of two elementary gases, together with small but appreciable quantities of two other gaseous bodies, products of combustion; beside other gaseous bodies of various kinds, in nearly inappreciable quantities, the latter varying somewhat in character in inhabited localities. Its substance, as a

whole, is a compound gas of nearly uniform composition known as the AIR.

The air, when uncontaminated by local causes, has been found by the most painstaking and careful observations, in all parts of the earth, and at all heights, from the level of the sea to the top of the highest mountains reached by man, or the greatest elevation attained by the balloon, to have identically the same components. Omitting the two smaller constituents, in 100 volumes of air there are 79·1 of nitrogen and 20·9 of oxygen, or in 100 parts by weight there are 76·9 of nitrogen and 23·1 of oxygen; oxygen being heavier than nitrogen in the proportion of 16 to 14. These proportions differ a little from a chemical compound of four parts (weights) of nitrogen to one part of oxygen, and beside this difference it must be stated there is certainly no chemical combination of the gases in air—they are simply intermixed. All gases or gaseous bodies mix with each other indefinitely and perfectly, whatever may be their relative densities or weight, a difference in the most extreme case of over 250 to 1, and they never separate from each other, wholly or partially, except by condensation of some of them from a gaseous form (or vapor) to that of a liquid by reduction of temperature or increase of pressure or both.

Beside nitrogen and oxygen in the air, there is always present carbonic acid and vapor of water. Of the carbonic acid the quantity is quite variable, but very small in all cases. Pure country air has an average of from 3½ to 5 parts by volume in 10,000—4 parts being considered by most physicists as a proper quantity to adopt as appertaining to *pure* air. Four per cent. of one per cent. may convey the idea to readers. While the quantity of vapor of water present is yet more variable, as it depends on the temperature of the air at the time as well as upon the locality, not only where the air may be taken, at any place of observation, but where the air came from, by the winds, to reach that place. The quantity of vapor of water present in air is called its humidity, and air is said to be saturated with humidity when it holds as much vapor as it can without its condensing into water as a liquid.

We commonly know vapor of water as *steam*. At 212° and under the ordinary pressure of the atmosphere (which we will speak more about hereafter) water boils, and if the temperature is maintained, it will all boil away in the air. But vapor of water exists in contact with water without boiling, at all temperatures, and if the pressure of

the atmosphere which rests upon it is taken away, water will boil at any temperature whatever, dependent on the extent of relief of pressure. It is a curious truth that water only exists as a liquid because it is held down by the pressure of the atmosphere upon its surface.

The natural temperatures of the climate we live in, omitting extremes, are, say, from 10° to 85° Fahrenheit. The quantity of vapor of water in each cubic foot of saturated air (at 30 inches of barometric pressure) has been ascertained with great care by eminent French physicists. This quantity is very small in any case, being only 4 per cent. by volume at 85°, and it falls off rapidly as the temperature falls; at 65°, or 20° fall of temperature, only half as much, or 2 per cent., can exist; at 45°, another fall of 20°, but 1 per cent. is found; at 28°, but $\frac{1}{2}$ per cent., while at 10° only $\frac{1}{4}$ of a per cent. of the volume of air can be invisible aqueous vapor. So much as even these small quantities do not exist in air generally, as the air which has derived its moisture from water or damp surfaces will, from the action of currents and winds, ascend to the upper, colder atmosphere, where it will deposit the same moisture by condensation, into clouds, with rain, hail or snow, when great quantities of moisture are condensed, and much loss of heat accompanies the position of the cloud as regards its elevation from the surface of the earth. The course of the winds will bring this dried air again near the surface at another place; so that the humidity of air in our country may at any time be only 30 or 40 per cent., or even less, of what would constitute saturation for air of the same temperature. The average humidity of the Eastern States is from 60 to 70 per cent. of complete saturation. The degree of saturation is measured by the *dew-point*, which is the temperature that is indicated by a thermometer, artificially cooled until a deposit of dew or condensed water appears on the bulb.

Air, as it is found in the neighborhood of our cities, and in the seasons of growth in the country, generally has very small quantities of other gases in its composition. The most general are ammonia, sulphuretted hydrogen and sulphurous acid gas, with numerous others of local derivation, especially near factories; but the quantities of such impurities present in the open air are even smaller than those given for carbonic acid or vapor of water, so that fresh air everywhere can be held, as before stated, to be mainly nitrogen and oxygen. Only the most delicate tests, where the hundredth of a per cent. is a unit, serve to measure the quantities of gaseous bodies vitiating air.

Beside the gaseous impurities referred to, there exists always in air of inhabited regions very small quantities of floating organic matter, composed of fragments of organic origin, vapors of the same source, like odors, for instance, microscopic germs or living organisms, together with dust of minerals or metals, smoke, etc., forming an insignificant part of the atmosphere, nearly inappreciable in amount by weight or measure, but of the greatest importance in effect upon the air of ventilation, as will be made to appear further on in this paper.

The main chemical characteristics of the gases in air are as follows: Oxygen is the most abundant element in nature. It forms, as stated, one-fifth of the atmosphere; it also is eight-ninths of the substance of water, and about one-half of all solid bodies of the earth—at least, of the crust of it so deep as we can investigate its formation. In its free state, and its existence in the air mixed with nitrogen can be considered free, it combines chemically with nearly all other elementary bodies. This combination is attended by evolution of heat, and is known as combustion.

Nitrogen, which is the chief constituent of the air, has few inorganic chemical combinations with other elements. It is an essential and considerable part of all animal tissues which are composed mainly of carbon, hydrogen, oxygen and nitrogen, and also an essential but very small part of vegetable tissues which consist principally of the first three bodies in the list.

Carbonic acid is the product of combustion of carbon, where two and two-thirds parts of oxygen by weight enter into combination with one part of carbon, also by weight, and form a colorless gas, about one and a half times heavier than air in equal volumes. It results from the burning of fuel—carbonaceous materials, either recent vegetable growths or the fossils of former vegetable growths—and from the slow oxidation of organic tissues called decay, beside being the chief product of respiration. Volcanic action, as well as some processes of combustion which take place in various localities under the surface of the earth, evolves large quantities of carbonic acid. On the other hand, while these sources of carbonic acid are in constant action, nature is restoring the equilibrium of condition; as all vegetable growths are absorbing carbonic acid, assimilating into wood tissues the carbon, and setting free the oxygen. It cannot be said, however, that the condition of the air is dependent upon vegetable growth to keep

down the proportion of carbonic acid, as it has been estimated that if the vegetable growth of the earth were to cease for two thousand years the effect of respiration and combustion in vitiating the air could only be detected by the nicest chemical analysis.

Carbonic acid is an innocuous gas, quite harmless to animal life except when it is substituted for oxygen in the air for breathing, and except in so far as its presence in large proportions interferes with the natural secretion from the lungs and possibly from the skin.

Vapor of water is the product of the combustion of hydrogen where eight parts of oxygen by weight combine with one part of hydrogen (the volume of the one part of hydrogen being twice that of the eight parts of oxygen). It is a colorless vapor or steam, about five-eighths as heavy as air, in equal volumes. When condensed as liquid water it is the chief constituent of organized bodies, forming the greater part of their weight. Water also plays an important part in the mineral kingdom as the water of crystallization of many minerals. Many substances dissolve in water. All animate creatures who live upon the surface of the earth require water as a liquid to drink, but the presence of vapor of water in the air does not seem to be absolutely essential to the existence of animals—except, perhaps, it may afford a mitigation of the extreme heat of the sun's rays as they shine through our atmosphere. But, on the other hand, all vegetable life demands, as a primary necessity, considerable vapor in the air, and in a warm saturated atmosphere it grows and thrives with the greatest luxuriance. Like carbonic acid, aqueous vapor is harmless to animal life, except when present in so large quantities as to interfere with natural secretions; but, as it condenses from air, at any usual temperatures in the habitable part of the globe, until the quantity of water present cannot exceed four to six per cent., the danger of such interference is almost entirely removed.

Having discussed the constituting elements of *air* and their characteristics as chemical bodies, some of the physical properties which bear important relations to ventilation and heating may next be noticed. The three conditions of material substances are gaseous, liquid and solid. An ideal perfect gas is perfectly fluid and perfectly expansible or compressible; relief of pressure or the addition of heat, with permission to expand under the same pressure will cause an indefinite enlargement of volume proportionate to the pressure or heat, while increase of pressure or abstraction of heat under constant pres-

sure produces proportionate reductions of volume also indefinite in amount. Although the discoveries of the past three years have rendered it nearly certain that no gaseous body whatever exists which at some pressure or temperature does not lose its gaseous form and become liquid, yet within the range of temperature and pressure of nature on the surface of the earth, the air may be treated as conforming to the ideal laws of a perfect gas.

It is not the less a material substance that *air* is a gaseous body. Its weight for a given volume, under a given pressure and at a given temperature, is well known. Thus at the pressure of 14·7 pounds upon a square inch and at a temperature of 32° F., one cubic foot weighs 0·0807 pound; or 12·4 cubic feet weigh one pound. Now, this pressure of 14·7 pounds on a square inch is the atmospheric pressure found to exist on the surface of the earth, and is the equivalent to 2116·3 pounds on a square foot. If 12·4 cubic feet weigh one pound, it would take a column of air of 26,227 feet to exert the load of 2116·3 pounds on the square foot, or very nearly five miles high. But the air changes in density as the pressure is reduced; or, in other words, as weight of the column of air becomes less and less towards the top, the volume of each cubic foot increases, so that the atmosphere is really 40 to 45 miles in height in place of the 5 miles which would exist if it had a uniform density.

The pressure of air is measured by the barometer, and 14·7 pounds to the square inch corresponds to 29·92 inches of a column of mercury in the mercurial barometer. This instrument may be briefly described as a glass tube about three feet in length, with one end closed, which tube having been filled with mercury when the closed end was downwards, is reversed into a shallow cup also holding mercury; when the column in the tube will leave the upper or closed end (and form a vacuous space) and descend into the cup, so far as the pressure of the air on the surface of the mercury in the cup will not support the column. There is found to exist at any place not much elevated above the level of the sea from 28 to 31 inches of length of this column; or, in other words, of difference in height of surface of the mercury near the top of the tube and that of the open cup at its foot. These three inches of variation of barometrical height are the limits of usual variation of atmospheric pressure.

The volume of air under any given pressure is much affected by heat. In common with most gases (and probably of all where the

temperature of the gas is considerably above the point of liquefaction), air expands or contracts $\frac{1}{491}$ part of its volume for each degree (Fahrenheit) of temperature above or below the freezing point. This change of volume is the great natural agent in promoting that circulation of the air and distribution of the heat from the sun which makes our globe habitable. A correct appreciation of its effect upon the air may be had by examination of the following table, which includes only a few usual atmospheric temperatures. The same laws, with small modifications, govern the volumes and densities of air to the highest temperature of combustion :

Temperature of dry air, degrees Farenheit	0°	10°	20°	32°	40°	50°	60°	70°	80°	100°
Volume of same weight, of air under the same pressure	459	469	479	491	499	509	519	529	539	549
	0·935	0·955	0·976	1	1·016	1·037	1·057	1·077	1·098	1·138
Density for constant volume	1·070	1·047	1·025	1	0·984	0·965	0·946	0·928	0·911	0·878
Weight per cubic foot-pounds	0·0863	0·0847	0·0828	0·0807	0·0794	0·0779	0·0765	0·0749	0·0735	0·0709

If, however, the pressure upon any given volume of air becomes greater or less, its temperature will then be found to have increased for the greater pressure and to have diminished for the less pressure. It results from this that the air upon the top of mountains, where the barometric pressure is greatly reduced, is found to be much colder than at their feet, until at the elevation of from four to five miles above the elevation of the sea a region of perpetual frost, even in the torrid zone, is reached.

There are two recognized standards of measurement of heat of substances. The first is that of the intensity or temperature. All bodies of unequal temperatures possess a tendency to equalize their temperatures by transfer of heat between themselves, when such bodies are either in actual contact (in which case the process is called conduction or convection), and also when they are in some degree in proximity to each other (in which latter case the process is denominated radiation), some, if not all, of the heat rays being found to pass through most gases and through some solid bodies. Such gases or bodies are denominated diathermanous. As substances generally expand by the increase of their heat and contract with its decrease, the extent of this

change of dimension between certain temperatures determined by natural phenomena has been used as a measure. The phenomena referred to are the freezing and boiling of water, and the temperatures communicated to thermometers (heat measures) by water at the freezing or boiling point (under defined atmospheric conditions) established limits for a range of expansion, which range is divided into parts called degrees. Three scales of division have had practical use, but one of them (Reaumur's) of 80 parts may be considered as superseded at this time. The other two are: first Fahrenheit's, where 180° of equal expansion are made between freezing and boiling, and where the freezing point is called 32°, and the same rate of equal expansion (contraction in this case) is carried downward below the freezing point to an imaginary zero; bringing the boiling point 32° + 180° = 212° above zero; and the second, Centigrade, where the freezing point is called zero, and 100° are spaced off from zero to the boiling point. By the English speaking nations the Fahrenheit scale is used as a popular scale almost altogether, and to a great extent as the scientific one. In other countries the Centigrade scale is in general as well as scientific use, and the next fifty years will probably witness its universal adoption in all countries. The mercurial thermometer is too well known to need description. The principle of measurement of temperature by the expansion of a body by heat is extended above the boiling point by degrees of supposable equal values to 10,000°, 15,000°, 18,000°, the last being the theoretic heat of carbon burning in oxygen, and is carried below the freezing point in the same way to the lowest temperature of existence in nature, and *to the utmost cold supposed to be possible*. The contraction of gases by removal of heat of one degree Fahrenheit, was stated to be $\frac{1}{491}$ part of the volume at 32°. Now, if it be imagined that the contraction were carried on for 491°, the gas must obviously disappear at the next diminution. This imaginary temperature of 32° — 491° = — 459° F., has been deemed the zero of absolute temperature, and it has been found that by adopting this supposed value in computations, the laws of expansion and elasticity of air, or gases, together with those of accompanying heat, can be expressed satisfactorily.

The second standard of measurement of conditions of heat refers to the quantity of heat which may be taken up or given out in effecting changes of temperature in the various substances. For this purpose water is again selected as the means for establishing a thermal or heat

unit. The English heat unit is taken as the heat appertaining to one pound of water heated one degree Fahrenheit. The foreign and scientific heat unit is the heat belonging to one kilogram of water heated one degree Centigrade, and is 3·97 times that of an English heat unit; but the English heat unit continues to be used in most treatises on applications of heat in the English language, and will be the only one referred to in this paper.

The specific heat of a substance is that quantity of heat, expressed in heat units, which must be transferred to or from a pound of that substance to effect a change of temperature of one degree. The quantity varies greatly for different bodies, and varies also in some measure at the different points in the scale of temperature in most of them. In the latter regard, however, the variation is so small that we can accept certain values which have been ascertained by experiment, and will be found in tables of specific heats of substances in books on physics as sufficiently accurate for practical purposes. For gaseous bodies the uniformity of specific heat at different points in the scale of temperature is more closely preserved than for solids or liquids, but these bodies are found to have *two values* for specific heats: one for the increase of temperature of one pound of gas, one degree, where the gas is permitted to expand under constant pressure; and the other, where the gas is enclosed so that, in place of expanding, the pressure increases in accordance with a certain law of elastic force dependent upon the addition of heat. Thus the specific heat of air, under constant pressure, is 0·238 heat unit; that is, 0·238 pound of water, losing one degree of heat, will impart to one pound of air (about $13\frac{1}{3}$ cubic feet at 70°) one degree of heat, while the volume of the air will have increased, under constant pressure, $\frac{1}{529}$th part. On the other hand, the specific heat of air, with constant volume, is 0·169 heat unit only; one pound of the air retained (to $13\frac{1}{3}$ cubic feet in the supposed case of 70°) in its original volume will be heated one degree by 0·169 pound of water losing one degree. This value of specific heat for constant volume does not apply to all gases, although nitrogen, oxygen, hydrogen nearly conform to the figures given.

The specific heat usually quoted and applicable to the theory of heating and ventilation is that of constant pressure, or 0·238 heat unit for air.

To complete this statement of the effects of heat, latent heat must be mentioned. Whenever any material substance passes from one of

its three conditions—gaseous, liquid or solid—to another, heat units are absorbed or given out, often in enormous quantity, without any apparent change of temperature of the transformed substances. Thus water at 212° requires the addition of 966 heat units to transform it into steam of 212°. Evolution or absorption of latent heat also accompanies chemical combinations.

One more property of heat should be named. Heat is a measure of force expended or utilized, and one heat unit represents the force of lifting 772 pounds to the height of one foot = 722 foot pounds.

Resuming the consideration of the physical properties of air.

The beginning of this paper mentions that *very* small quantities of various substances which cannot be considered as gaseous bodies were to be found in the air of habited or habitable places. Generally, especially in towns or cities of the temperate regions where manufacturing callings or the comfort of the inhabitants demand the use of fires, the main portion of these impurities of air consist of dust of minerals, and metals smoke (which is principally dust of charcoal or mineral coal), and similar bodies reduced to so fine a state of powder that the adhesion of air to the particles prevents their settling in it, except so slowly that they may be said to float, and as floating bodies will have been dispersed with the currents; and they may be in some measure diffused by the inter-currents of diffusion of vapor of water or carbonic acid or other gaseous bodies with which they were particularly associated in their origin. The effect of this adhesion of air to particles of matter can be appreciated by stating that, but for it, rain drops would reach the ground with an acquired velocity equal to that of shot from a gun; and by it the impact of hailstones, even of the largest size, is modified so far as to reduce the injury they occasion, to the destruction of glass, defoliation of trees, and similar results, such as might happen from stones thrown by the hand. In the case of a hail storm the phenomenon is produced by a violent ascending current of air, which at the height of five to eight miles reaches the region of perpetual frost, where the hailstones are formed, and the stones descend through and against a current of from 20 to possibly 200 miles per hour.

The dust referred to, after all, except in localities where decidedly injurious fumes are generated, or in work-shops, or any business where a volume of dust or smoke is created, or the exposure of the workman to breathing it, is improperly guarded against, can only be considered

rather as objectionable than noxious. This question of means of prevention or removal of dust or smoke will come up again when considering the practical applications of ventilation.

Odors or smells constitute palpable impurities—vitiations or, perhaps, quite harmless portions of the air. Some of them are unquestionably dusts of solid bodies; others are definite chemical gaseous bodies; others, again, are seemingly in combination with the vapor of water, in which they are dissolved in the atmosphere. Dust of organic matter; small particles of the skin, and fatty matters detached from the skin are abundant in the air of all houses; in the air of the streets similar exhalations from horses and other animals can be detected; in the air of the country, vegetable particles predominate. These dusts are in every stage of decomposition. When first separated from their source they are generally undecomposed and inodorous, but sometimes, like pollen, they possess the power of affecting the sense of smell; in moist atmospheres they rapidly decompose, having become the soil for numerous microscopic growths which accompany, and it is now satisfactorily determined, occasion the decompositions.

It must be borne in mind that the *quantity* of organic matter described so briefly in the preceding paragraph is exceedingly diminutive as compared with the volume of air. Light as air is, not the one hundred millionth part of its volume for pure air on high ground, or about one five millionth part in a crowded railway carriage (as deduced from figures of Dr. Angus Smith), is organic impurities. Yet to a very small portion of this very small portion of the air is now attributed, by the best authorities, the greatest danger from breathing of vitiated air.

It has been long known that fermentation or some similar action accompanies most, if not all, organic decompositions; and it was reserved to Drs. Schrœder and Pasteur, especially to the researches of the latter to demonstrate that countless germs of vegetables and infusoria exist in the air, which will develop wherever suitable organic matter is found to support their growth. These views were combated by several writers, some of whom, admitting the fermentative growths, supposed or advocated their origin by spontaneous generation. But this last view has now been satisfactorily refuted by many experimenters; Prof. Tyndall's investigations being very conclusive in showing that with pure air no change of the most decomposable substances and solutions commences. Prof. Tyndall's test for the purity of air is to

allow a sunbeam to shine down a tube containing air which has been filtered through cotton-wool. The smallest particles of dust, germs or grains beyond the power of discernment by the microscope, are illuminated by this test to brilliant points, until their presence becomes evident to the observer.

It is positively known that different chemical changes are brought about by different germs. Alcoholic or acetous (vinegar) fermentation, lactic acid, butyric acid, etc., proceed from different vegetable or organic growths. It finally seems probable that the whole train of epidemic diseases owe their origin to atmospheric germs which find their suitable organic matter for growth in the human system.

The organisms themselves are minute almost beyond the limits of conception. One of the most common, the Bacterian termo, which is a living organism, "has a wasp-formed body, each enlarged part being about $\frac{1}{14000}$ of an inch long and $\frac{1}{15000}$ of an inch wide, joined by a filament of extreme thinness, about $\frac{1}{11000}$ of an inch long; and has a 'flagella' $\frac{1}{7000}$ of an inch long at each end, not over $\frac{1}{1000000}$ in width, and so thin as to be undiscernible in the side view. The flagella is lashed incessantly." What must be the magnitude of the germs of this perfect organism? Beside the germs of the Bacteria, those of Vibrios, Mycodermes, Mucidines and Torulae are given by Pasteur and others, as of known organisms whose characteristics and purposes are well established. The almost immediate occurrence of fermentation of some particular kind in each fermentable liquor when exposed to common air in any place, and the complete suspension for an indefinite time of fermentation when only *pure* air, tested as Tyndall has described, comes in contact with such liquor, must be accepted as proof of the agency and universality of atmospheric germs. Heat far beyond the boiling point will not kill certain of these germs, and some defy certain chemical agents which are destructive of life generally.

Each new consideration of the subject of the effect of air on the health of mankind, adds more evidence or reasoning to support the view that gaseous impurities are fraught with but a very small portion of the danger which appertains to the organic vitiations.

"It should be here remarked that the basis of the *germ theory* is, that all natural decompositions of organic substances (in contradistinction from decompositions, by fire or chemical action) are growths. Growths in themselves healthful of their kind (same as the lion or the mosquito

have healthful lives—the oak or the weed have healthful growths). Each growth, it may be descending in the scale of growth and ascending in another path, to be again reduced. A cycle of vitality from the lowest organism through the vegetable and animal life to man, and returning by a retrograde course (if it be retrograde) to a new train of existence."—[Excerpt from a report on the sanitary condition of a building, by the writer.]

The next step in the course of the investigation is to consider the quantities of air requisite for the health and comfort of the human occupant of the ventilated room or place, for a given interval of time. These quantities are composed of several distinct requirements, the most essential of which are the *necessities* of respiration and of absorption of the vapors of transpiration ; inadequate supply of air for these purposes resulting in suffocation. Another requirement for dwellings, is the supply of air for fuel or for lighting, in the latter case for the dispersal of heat in some degree. But all these requirements, essential as they are, call for quantities of fresh air, quite insignificant in amount to the demands for dilution of the products of respiration, transpiration and combustion, so that the air of a dwelling shall have that degree of purity which is conducive to healthful residence.

The healthy adult man, in still life, in an atmosphere of normal condition (which, in our climate, may be taken at the temperature of 60° to 70°, with 80 to 70 per cent. of humidity), whether awake or asleep, inspires on an average, 30 cubic inches and expires a corresponding quantity, slightly dilated by heat and by chemical change at each respiration, and he breathes 16 times each minute ; giving 480 cubic inches (or 0·278 cubic feet) of air demanded each minute. Let it be supposed that the air inhaled in pure air with 0·0004 its volume of carbonic acid gas, and to have the temperature of 70° with 70 per cent. of humidity ; in such case, the exhaled breath will have a temperature of 90° ; and the entire volume will have been increased about $7\frac{3}{100}$ per cent. ; it will be saturated with moisture, which will form over $4\frac{7}{100}$ per cent. of its volume ; while the increase of weight, by taking up moisture and carbon from the lungs, will have been only $3\frac{6}{10}$ per cent. ; at the same time, from the 20° rise of temperature, the density, as a whole, will have been reduced $3\frac{2}{10}$ per cent. ; the carbonic acid gas in exhaled air will be $3\frac{8}{10}$ per cent. of its volume ; and the quantity of oxygen of the inhaled air will have been reduced about $19\frac{3}{4}$ per cent.

The average respirations of an adult for one minute will tabulate as follows:

Table of constituents and products of 16 respirations, of 30 cubic inches each.

Constituent gases.	Inhalations at 70° temperature and 70 per cent. humidity. Wt. = 0·07413 lbs. per cubic foot.			Exhalations at 90° temperature saturated with aqueous vapor. Wt. = 0·07234 lbs. per cubic foot.			Ratio of exhalations to inhalations.	Change of weight by the act of respiration.
	Wts. lbs.	Vols. cu. ft.	Vols. per cent.	Wts. lbs.	†Vols. cu. ft.	Vols. per cent.	Vols. per cent.	Wts. lbs.
Nitrogen	0·015998	0·2181	0·7851	0·015998	0·2264	0·7910	1·0381	0
Oxygen	0·004542	0·0548	0·1973	0·003645	0·0450	0·1584	0·8200	−0·000897
Aqueous vapor	0·000221	0·0047	0·0169	0·000629	0·0146	0·0471	3·0787	0·000408
Carbonic acid	0·000013	0·00011	0·0004	0·001246*	0·0113	0·0380	102·75	0·001233
Total	0·020774	0·2774	1·0000	0·021418	0·2973	1·0000	1·0700	0·000644

The difference of the changes in weight of carbonic acid and oxygen in the last column of the table = 0·001233 — 0·000897 = 0·000336 lb., represents the weight of carbon taken from the lungs, being an actual combustion of so much carbon in the system (as appears from the breath) each minute.

To the casual reader it would appear that the computations of the above table were needlessly extended, especially when it is considered that the data are assumed averages, and not absolutely accurate; but the fact is, that the vitiations of air bear so small a part in relation to its volume, that it requires long lines of decimals to express them at all; and accuracy in the last figures is demanded, to make the proportions of what are admitted vitiations, or rather are admitted as the accompaniment or vehicle of vitiations, appreciable.

Beside the air needed for respiration, an uncertain quantity is both

* The quantity of carbonic acid exhaled, adopted in this table, is deduced from the experiments of Dr. Edward Smith, Proc. Roy. Soc., 1859.

† This column of vols. cu. ft. was obtained thus:

	Weight, Lbs.	Weight per cu. ft. at 32° Lbs.	Cu. ft. at 32°	Cu. ft. at 90° saturated Cu. ft.
N_2	0·015998	0·0723	0·2181	0·2264
O_2	0·003645	0·0894	0·0409	0·0426
H_2O	0·000629	0·0466	0·0135	0·0140
CO_2	0·001246	0·1142	0·0109	0·0113
	0·021418		0·2864	0·2973

needed and vitiated by transpiration. A constant exhalation of carbonic acid gas transpires from the skin; by no means so large in quantity as is emitted with the breath, but probably one-fourth or one-fifth as great. The regularity of transpiration nearly equals that of respiration. Accompanying this, it is probable that an absorption of oxygen, corresponding to the equivalent of oxygen in the carbonic acid, takes place. The best authorities do not seem to have found the expired air from the lungs to have lost more oxygen than the carbonic acid exhaled required; and as all authorities assert the exhalation of carbonic acid from the skin, it follows of course that the supply of oxygen to form this carbonic acid must be absorbed by it. The phenomenon of interchange of gases occurs with the cutaneous secretions, similarly, if not equal in extent, to what happens in the so-called revivification of the blood.

The exhalation of moisture from the skin, however, is a very variable quantity as compared to what exhales from the lungs. The internal temperature of the human being is perhaps 98°, while the comfortable and healthful temperature of the air in contact with the skin is from 10° to 30° below this point; the degrees of heat of the air, varying greatly with its hygrometric condition—or, in other words, with the proportion of moisture present. The loss of heat from the evaporation of moisture from the skin into the air, being far greater than the cooling effect of the air itself. In temperate regions, also, a large part of the person is protected by clothing, whereby the temperature of the air next the skin, under the clothing, is elevated, until, for instance in our climate, an admitted summer temperature of 70°, accompanied by 70 per cent. of humidity, is the standard of condition for the active man, although a higher rate of humidity (80 per cent.) is perhaps more conducive to luxurious comfort and ease.

Be this temperature and corresponding moisture condition what it may, the fact remains that by insensible perspiration, as it is called, a large amount of moisture is exhaled from every human being each day, hour or minute, and this moisture is laden with organic matter; and a certain quantity of fresh air is needed to absorb and dilute it, and the accompanying organic vitiations.

Some observers, after considering the relative quantities of liquids and solids taken as food and excreted daily, have estimated that from 1·5 to 2·5 pounds of liquid, must, on an average, be dissipated from the system of an adult in active life in the time named. The mean of

these quantities may be accepted as the loss by evaporation from the lungs and skin in occupied places = 2 pounds; when about 0·0014 pound will pass from the skin and 0·0004 pound is evaporated from the lungs each minute. On the other hand, the exhalation of carbonic acid, as before stated, is not nearly as much, probably, from the skin as from the lungs.

It should also be stated that a small quantity of nitrogen has been found to be absorbed by the lungs, and a very little ammonia is either given off, or is formed almost instantly, by decomposition of some of the emitted organic matter. These vitiations are, however, only appreciable by delicate observations, which observations have given such discordant results as to throw doubt on the experiments as bases of theory. Still it may be asserted that nitrogen is the natural and fundamental part of the atmosphere for the types of animal life on the face of the earth, and that no other gaseous body can be admitted to replace it; while oxygen, in the proportion in which it is always found in the air, is the necessary and sole active agent in sustaining life.

From all that has been said in this article, it will be evident that, for the purpose of breathing solely, only a little more than one-quarter of a cubic foot of air is needed each minute by the average healthy adult. Perhaps this quantity will be raised to one-third of a cubic foot when the air of transpiration is included. And that there must flow away from the person, by respiration and transpiration combined, also each minute, 0·0014 pound or 0·03 cubic foot of vapor of water at 70°. If these quantities of exhalations, small as they are, are positively and absolutely removed, and fresh air substituted for the first of them, a perfect ventilation will have ensued.

The sole mode of removal possible, is by diffusion and dilution. The purity of air in any occupied place can only be relative. A certain quantity of exhalations in a given time will mingle with a certain quantity of fresh air supplied in that time (neglecting the loss by transfusion through walls, as septa, of some small quantities of carbonic acid and vapor of water, the latter especially when the exterior dew-point is low); and a definite ratio of the constituent parts of the air of any occupied place, will eventually be established.

It is customary to attempt the establishment of the proper quantity of fresh air by the ratio or percentage of carbonic acid *admissible* in a habited room. If it is supposed that twice the quantity of carbonic

acid is admissible in a continuously occupied room over that existing out-of-doors in fresh air, then 99·1 times as much fresh air as is needed for respiration, etc., must be supplied to dilute the exhaled air, a proportion which gives 33 cubic feet of air to each person per minute, with a result of 0·0008 volume of carbonic acid present.* The quantity of carbonic acid in any closed room will be further reduced by some diffusion at cracks of doors or windows.

Another method for determining the quantity of air needed is based on the diffusion of vapor of water. The supposition that the hygrometric condition of the air is to be elevated, say 5 per cent., will, if the temperature of the air of the room is 70°, allow the diffusion of 0·000056 pound of vapor per cubic foot of air, or for the 0·0014 pound of vapor emitted from the person each minute, 25 cubic feet of air to each person per minute.

Either of the above ways for *computing* the requirements of ventilation are purely empirical and founded on no reasonable or natural demand. The quantities they give, however, are about those adopted by the best authorities as the least for healthy persons, while double these quantities are required in hospitals. The air of dwellings and of hospitals has *proved* to be pure to the sense of smell with the quantities above stated, if the distribution and removal has been well arranged; with less quantities this is not the case. After all, the standard of purity of air is founded on the *perception* of an almost infinitesimal quantity of organic matter, and upon results of tests of health of dwellings, etc., and not upon the reasoning of the chemist.

Following the requirments of defined quantities of air for personal ventilation of the inhabitants of rooms, further demands for the purposes of supply of air to fuel used at times in heating them, and for the consumption of gas, oil or other material producing light by burning, should be investigated. What is needed for warming, however, may be more properly considered when discussing the heating of dwellings or other places, only remarking here that the quantity relative to what is requisite for *dilution* of the air of breathing, or for

* These figures are obtained as follows: Accepting the air for respiration at 0·2777 cubic foot at 70° per minute, and adding one-fifth for one for transpiration, we have 0·3331 cubic foot per minute. With the volume of air which shall give the requisite excess of 0·0004 CO_2 added to the normal air, the effect of raise of temperature may be neglected, when the ratio of volume of CO_2 in the exhalations to that in the inhalations becomes 1 to 99·1, in place of 1 to 102·7, which was the ratio for an increase of temperature of 20° (70° to 90°) $\frac{1090}{11} = 90\,1 \times 0{\cdot}333 = 33{\cdot}0 + 0{\cdot}333 = 33{\cdot}33$ cu. ft.

reduction of heat, is so small (while the diluted or vitiated air has sufficient oxygen not to be impaired for supporting combustion of fuel), that it drops out of consideration in the question of volumes of air to be furnished. And there is left for consideration at this time only what supplies of air are requisite for gas burners and oil or other lights.

The gas burner in common use will burn from three to six cubic feet of ordinary coal gas per hour; each cubic foot of such gas consumed will take up the oxygen of 6·1 cubic feet of air, and will require the presence of about 12·2 cubic feet of air at the point of ignition, in order to effect complete combustion. This double supply of air is found in practice necessary for the combustion of fuel of all kinds, under usual conditions, in air of usual temperatures, and the escaping gases, when burning hydrogen or carbon, will consist of vapor of water and carbonic acid (if the carbon is entirely burned), as novel chemical products, together with free nitrogen, and as much free oxygen as was not taken up in the chemical combinations. In the same way it was noticed that the air expired in breathing had been deprived of only about one-fifth of its original oxygen, and it was then accepted that such expired air was unsuitable for a new respiration.

The average gas burner in general use may be assumed to burn $4\frac{1}{2}$ cubic feet of gas per hour, and the quantities reduced to the unit of a minute, so as to be comparable with the estimate for respiration as previously established, give 0·075 cubic foot of gas, which takes up, by chemical combination, the oxygen of 0·46 cubic foot of air, and needs 0·02 cubic foot of air to accomplish the burning. The products of combustion, together with and including the free nitrogen and oxygen, forming the *gases of combustion*, have the volume of 0·97 cubic foot, when reduced to the temperature of 70°, at which temperature all the foregoing figures have been taken.

Thus it is seen that the demand of air for a gas burner (burning $4\frac{1}{2}$ cubic feet per hour) is very nearly three times as great as that for the respiration and transpiration of an adult man in still life. If, however, the same rules for determining the quantity of air for dilution of the carbonic acid or vapor of water generated, are applied to gas burning as were used in the case of respiration, we have the following results. A $4\frac{1}{2}$-foot burner will generate 0·0455 cubic foot of carbonic acid each minute, whence 114 cubic feet of fresh air will be needed in the same time to dilute this carbonic acid so that in the

resulting mixture 0·0008 volume of carbonic acid (twice the normal quantity) will be present. The same burner will produce 0·00475 pound of vapor of water each minute, which calls for 85 cubic feet of air, if the condition of adding 5 per cent. to the humidity at 70° is thought to be the standard for attainment.

In the act of respiration, as discussed in the last number, it was shown that 0·000336 pound of carbon was consumed in the system, as measured by the expirations, each minute. The estimate of this quantity is increased by some emitted carbonic acid by transpiration; adding, as before assumed, one-fifth, then 0·0004 pound of carbon can be accepted as consumed each minute. Whence, recognizing that 14,500 units of heat proceed from the perfect combustion of carbon into carbonic acid, it results that 5·8 units of heat will be produced.

From this quantity of heat is to be deducted the heat requisite to vaporize the exhaled moisture from the lungs and from the skin. The previous assumption of vapor emitted each minute, at 0·0014 pound, multiplied by 1062° (= the latent heat of vapor at 70°), gives 1·49 units of heat as absorbed in the evaporation, leaving 4·31 units of heat to be accounted for. What proportion of this heat is taken up by the labor of work, or in the functional demands of animal or mental life is very uncertain.

Taking the 30 cubic feet of air allotted in the last number for the requisite of ample ventilation of a person each minute, we have 30 × 0·0744 (the weight of one cubic foot of air, of 70 per cent. humidity, at 70° temperature) = 2·232 pounds of air at 70°; multiplying by 0·238, or the *specific heat* of air, we have 0·531 as the number of heat units demanded to heat the 30 cubic feet of air 1°. If it be assumed that all the heat unaccounted for—the 4·31 units—is expended in heating the 30 cubic feet, then the temperature of the 30 feet will be elevated a little more than 8°. It is not probable, however, that the amount of heat to be dissipated exceeds one-half the total, and possibly one-third is nearer the case. I think that the elevation of temperature of the surrounding air, when 30 cubic feet of air per minute is allotted to each adult in still life, does not exceed 3°, but am ready to admit that the grounds for this belief are too nearly a mere guess to be satisfactorily stated.

It is not unfrequent that in a crowded room, in warm weather, a number of persons will be collected together who will have been provided with not over 10 cubic feet of air per minute. On the supposi-

tion above, the temperature of such a room would be raised from 70° to 79°, presenting some probability of coincidence with facts. But the effect of any such elevation of temperature with the supposed limited supply of air, will be to increase the avidity of the air for moisture and to promote perspiration, which will again afford relief by the evaporation of water, and thus limit the proportion of heat given to the surrounding air to temperatures of endurance. As the temperature of air rises, the amount of heat given out by evaporation will increase until it even exceeds that given out by conduction or radiation, or by both combined.

The heat effects from a gas burner are as follows: taking the gas as having the usual quality of 14 to 15 candle power, the heat proceeding from such gas is very nearly = 622 units for each cubic foot of gas burned. [Gas of 14 to 15 candles is such that when 5 cubic feet are burned in a properly shaped burner, under ½ inch water column pressure, in one hour, the light given out will be equal to that proceeding from 14 or 15 standard spermaceti candles, each of which shall burn at the rate of 120 grains of spermaceti per hour.] This gives the hourly heat production from 4½ cubic feet to equal 2800 units, or 46·7 units to be dispensed each minute. The existence of this quantity of heat in combination with the gases of combustion as they arise from the flame, is one of the best established facts in physics, but its dispersal when these gases are diffused is scarcely reconcilable with the observed heat imparted to a closed room by a gas burner. The quantity of heat which will have disappeared by the diffusion is indeterminate. I am not now willing to admit that over one-third the heat which has been theoretically evolved, will have been imparted to the air of a room.

It has been customary to assume that 10 cubic feet of air per minute for each cubic foot of gas burned per hour should be supplied for *ventilation* of a gas burner. This rule gives 45 cubic feet of air for a 4½ foot burner. From such a burner, under such circumstances, the temperature of the air of the room being 70°, that of the air ascending from open burners will be 99° and that ascending from argand burners will be 128°, *on the supposition that none of the heat is wasted in diffusion*. Supposing only one-third of the heat to be imparted to the gases or air of dilution, the temperature of the ascending currents become 79½° and 89° respectively. The real temperature of the gases

rising from the flame of a gas burner, unmixed with air of dilution, may be stated at 2640°. Unless enclosed in a chimney of some kind, these gases rapidly mix with the air around them, until within two or three feet they fall generally below the boiling point. Still the current which reaches the ceiling of a room is generally much elevated in temperature, and spreads over the surface as a stratum, with little tendency to descend or to mix downwards, except by diffusion. This fact and the comparatively brief time of gas lighting, are great aids in meeting the difficulty from heating and from gases of lighting. Another thing must be borne in mind, that there are no organic impurities to be dispersed from the products of combustion. Discomfort, and in extreme cases even suffocation, may follow the want of ventilation of burners, candles or lamps, but disease, in a strict sense, cannot arise from this cause. It is clear that the test of proportion of carbonic acid present, as a measure of vitiation, does not apply to lighted rooms.

It must be noticed that the rate of supply of air for gas burning *i. e.*, 45 cubic feet per minute for a $4\frac{1}{2}$ foot burner, will eventually bring up the rates of carbonic acid in any room (in course of time, however large the room may be) to 0·0014 volume, supposing the normal fresh air to have 0·0004 volume in it, and supposing that no diffusion of carbonic acid occurs through walls or cracks, where air will not circulate as a current.

The ventilation of candles or lamps could be investigated with equal care to that which has been given to gas lights, but it is sufficient to say here that the average candle gives about one-fifteenth the light proceeding from an average gas burner, and about equals the ordinary hand lamp, with oil as the burning material. Either of these can be taken to have somewhat greater heat effects than gas of the same luminous value, and about 5 cubic feet of air per minute can be taken as the quantity needed for each candle or lamp. The carcel or petroleum argand lamps can be estimated at slightly less heat effect for given light production, and the smaller ones will equal two-thirds an average gas burner in this regard; such lamps will require 30 cubic feet of air per minute.

Applications of Electricity.—It has been sometimes thought that a copper cable of enormous thickness would be required to transmit the hydraulic power of Niagara falls to New York. Prof. Ayrton has shown that the whole power could be transmitted by a slender copper wire, provided that the wire could be thoroughly insulated. He has also shown that the only hindrance to receiving the whole power is the mechanical friction of the machines. It is therefore believed that immense machines, with continuous currents, with detached exciters or magneto-electric machines, driven very rapidly by steam engines, will hold an important place in the future transmission of energy. With such machines it would be possible to warm, to light and to give work-shops the power which is necessary to move all their machinery by means of an ordinary telegraph wire, thoroughly insulated and transmitting energy from great distances. Prof. Perry thinks that it will sometime become possible to see what is going on in remote places by means of electricity.—*La Lumière Electrique.* C.

Decomposition of Nitrates by Decomposition in the Dark.—In plants which grow in the dark, although there is a regular organization and development of stalks, roots and appendages, there is a persistent elimination of a portion of the material which was contained in the seed. The tissue of the organs which are developed under a shelter from light is firm and strongly impregnated with a liquid which has a sensibly acid reaction. The cotyledons are, therefore, provided with the principles which are necessary for the life of the embryo; but nocturnal vegetation is unable to fix the carbon of the carbonic acid which is in the air. Boussingault has experimented in order to find whether this impotence extends to the fertilizing substances, which the roots commonly draw from the soil; if, for example, nitrogenous compounds, such as the nitrates and the ammoniacal salts, are assimilated. He immediately found that if a measured quantity of these salts is added to a sterile soil a considerable portion disappears entirely, and only a part of the quantity is found in the growth. He therefore concludes that a soil which had been rendered entirely sterile contains traces of organic substances after vegetation in the dark. These substances are probably due to an excretion from the root, which exercises a destructive action upon the acid of the nitrates that were added to the soil.—*Ann. de Chim. et de Phys.* C.

Phosphor-Tin.—An alloy of tin with phosphorus is much used in Germany, especially in the preparation of phosphor-bronze. The mixture should contain at least 9 per cent. of phosphorus in order to secure a thorough incorporation of the tin. If more than 9 per cent. is introduced the excess oxidizes and is driven off by the heat. A mixture which contains 9½ per cent. of phosphorus satisfies the formula P_2Sn_5, corresponding to the oxide, P_2O_5.—*Der Techniker.* C.

Relations of Intensity in the Sodium Lines.—Chase's suggestion of a probable planetary influence upon spectral lines has been partially confirmed by the investigations of Dietrich upon the comparative intensity of the two sodium lines. Experiments upon different days showed differences of intensity, which furnish some indications of regular progression, and which show, in the opinion of the experimenter, that the lines do not consist of homogeneous light, but of beams of slightly different wave lengths. He proposes to continue the investigations for different flames, and to publish the results at no distant period. The variability which Lockyer first noticed was most strikingly indicated in the components of the sodium line.—*Wiedemann's Annalen.* C.

The Brin Process for Oxygen.—Boussingault first discovered the property of barytes to absorb the oxygen of the air at a certain temperature and to restore it at a higher temperature. Great practical difficulties have arisen, from the fact that the barytes rapidly become inert and require to be revived, and that the oxygen is usually very impure. Messrs. Brin Bros. heat the commercial sulphate of barytes, in special furnaces, with 25 per cent. of carbon, in order to form a sulphuret, which is dissolved in water and treated by nitric acid. A nitrate is thus obtained which, when calcined in a special furnace, gives a caustic barytes at a cost of 2½ francs per kilogramme (19 cents per pound). Then, by a proper preparation of the air in order to render it easily decomposable, by the use of pumps and ventilators or aspirators in order to facilitate the peroxidation of the barytes and extract the oxygen afterwards, and by the employment of special pyrometers for the automatic regulation of the furnace temperature, they are already able to produce oxygen, of great purity, at a cost of 62 centimes per cubic metre ($3.50 per 1000 feet). They think that by manufacturing upon a large scale the cost could be reduced to about one-fifth of this amount.—*Chron. Indust.* C.

Bronzing Copper.—At the Paris mint medals are bronzed by boiling them for a quarter of an hour in the following solution: Pulverized verdigris, 500 grammes; pulverized sal ammoniac, 175 grammes; strong vinegar, 160 grammes; water, 2 litres. An untinned copper boiler is used, and the medals are separated from each other by bits of glass or wood.—*Les Mondes.* C.

Chinese Acoustics.—A Chinese physicist asserts that the law, which is commonly accepted, and which states that the octave of any note may be produced by doubling the length of a musical cord or tube, is strictly true only for cords. He says that experiments with tubes of different lengths and diameters have led him to the conclusion that the ratio of length is as 4 to 9, instead of 1 to 2. The interest of the Chinese in foreign science is shown by the publication of numerous English and American scientific treatises in the Chinese language.—*Les Mondes.* C.

Conservation of Electricity.—Quantity of matter and quantity of energy are not the only magnitudes which remain invariable. Lippmann claims that quantity of electricity enjoys the same property. If we study any electric phenomenon thoroughly we find that the distribution of electricity may change, but that the sum of the quantities of free electricity never varies. If the electric charge undergoes a positive variation at certain points there is a corresponding negative variation at others, and the algebraic sum of all simultaneous variations is always zero. This law constitutes the principle of conservation of electricity.—*Comptes Rendus.* C.

Inverse Electro-motive Force.—LeRoux has published a simple process for showing an inverse electro-motive force in the voltaic arc. It consists in extinguishing the arc by opening the circuit and immediately re-establishing, by hand, the communication between the two carbons through a galvanometer. This shows the existence of a current going from the negative to the positive pole between the heated carbon points and in the contrary direction in the galvanometer. Hence arises the difficulty of illuminating two or more arcs in a continuous current, since it is necessary to overcome the same inverse force for each arc. Magneto-electric machines with alternating currents profit by the existence of this inverse current, and this is one source of their great advantage over ordinary batteries.—*Comptes Rendus.* C.

Magnetic Intensity of Iron.—According to Rinman's investigations the magnetic intensity of iron appears to increase with the increase of carbon. In three specimens of cast iron containing ·09, ·30 and ·57 per cent. of carbon the magnetic intensity was 5·5, 9·5 and 11, respectively.—*Berg- und Hütten-Zeitung.* C.

Velocity of Light in Quartz.—In investigations upon refraction, Kohlrausch found nearly uniform values in different pieces of any given substance, while Quincke subsequently asserted that there were great variations in the refractive relations of quartz, which appear to be somewhat dependent upon the age of the surface from which the total reflection was observed. He states that this difference reached two units of the second decimal place in some cases. Hallock has carefully examined a number of specimens, and the greatest difference that he found was less than one unit of the third place. This included the uncertainty of observation, which was equivalent to at least two or three units of the fourth place.—*Wiedem. Ann.* C.

Book Notices.

WORKING DRAWINGS, AND HOW TO MAKE AND USE THEM, designed for Industrial, Technical, etc., Schools, and Artisans desiring a knowledge of the principles of Pattern and Template making. By Lewis M. Haupt, Professor of Civil Engineering in the University of Pennsylvania, etc. 12mo. Philadelphia: Jos. M. Stoddard & Co. 1880.

Engineers and master mechanics of all kinds have greatly felt the loss of time and effort expended in instructing their apprentices in the very rudiments of their professions and trades, and have been amazed that even graduates of technical and scientific schools generally come to them with entirely erroneous methods of thought and practice.

The title and preface of this book give the impression that the author has done a good work in presenting a plan by which cadet engineers can be graduated capable of entering the draughting room and being immediately useful, with their imaginations trained to clearly conceive an object from a sketch or verbal description; with the technical knowledge of scales, pencils, instruments, inks and colors, and the manual skill to use them with neatness and dispatch; and with

some experience of the best American practice in regard to the making of working drawings, such as the relative arrangement of different views, the avoiding of unnecessary repetitions, the judicious use of sections, coloring, and shade lines, and particularly with a knowledge of the best distribution of dimension lines and figures, and with a neat and clear style of making them. An examination of the text will fail to confirm this impression. Instead of teaching "How to Make and Use Working Drawings," and of being "the connecting link between theory and practice," it is based entirely on the theory of Descriptive Geometry and is in direct opposition to the practice of American and English engineers.

Davies' "Descriptive Geometry," published in 1826 [in which the author prefers no claim to invention or discovery, but merely to publication in America, and states that the subject has been taught since 1817 in the U. S. Military Academy, and that, in France, it was then considered indispensable to the architect and engineer] is more clear, interesting and useful than the book under review, which is a diffuse elaboration of the same old system, as applied to the projection of points, lines and planes.

The best engineers have long since discarded these theories and methods, with their diedral angles, ground lines, projections, traces, etc., as tending to confuse a subject which is essentially simple; and, with a full knowledge of these old problems, analyses, constructions and rules, have adopted the common sense method of showing objects as they appear when viewed from different sides, and of placing these views on the paper in their natural relative positions: thus, a top-view is placed above a front-view; a view of the right-hand side is placed to the right either of the front-view or of the top-view; a view of the left-hand side, to the left; and a view from underneath is placed below. The advantage of using this method in drawing a complicated machine is very great. It brings the different views of the same part closer together, facilitating the projection of the points, and the comprehension of the drawing when finished. It is the natural, logical method of procedure. A workman, seeing on a drawing a front view of a horizontal engine for instance, and wishing to get the dimensions of the crank, other than those shown in this view, would naturally expect to find them on the end-view shown next to the crank-end and not on the one next the cylinder-end, precisely as he would walk

around to that end of the engine itself to see the thickness or length of bearing of the crank.

The result of the teachings of text-books, such as this, is that the graduates of our schools, on entering practice, have to acquire a new method of comprehension, and a new manner of delineation, which, although far simpler and more natural than the old, is obtained at a disadvantage, in that the old one was received from what they have been taught to consider standard authorities.

A subject which is so simple, easy and interesting as the making of working drawings, and which could be made a relief to the tedium of studies, should not be put in the form of problem, theorem, analysis and construction, but should be based on the experience and practice of eminent engineers and artisans, and should form a training for the common sense and good judgment of students, and a preparation for their entrance to the designing room and workshop.

Even for teaching theoretical descriptive geometry the engineer's system would be simpler and clearer, and the advantage of adopting it in our technical and industrial schools would be very great, because the training of the students would then be in harmony with their future work, time and trouble would be saved during their early apprenticeship, and they and their employers would be spared the difficulty of overcoming erroneous early impressions and prejudices. A text-book of descriptive geometry, based upon the method of thought and mode of procedure actually adopted by skilled draughtsmen, and supplemented by one on working drawings as they are really made and used in our large industrial establishments, would deserve to be studied in every scientific school and by every mechanic in the country; but a professedly practical book, like the one under review, which teaches obsolete methods and incorrect technicalities, must certainly be misleading and injurious to the people whom it was intended to benefit. W. H. T.

FRANKLIN INSTITUTE DRAWING SCHOOL.

The following is an abstract of Mr. Robert Grimshaw's remarks at the recent closing exercises of the school:

"I leave my own drawing-room for the purpose of doing myself the honor of attending the closing exercises of your drawing class; to

testify the interest I feel in instruction in drawing in general, and in the schools thus interested in particular.

"You have kindly asked me to speak in behalf of the art of drawing and of this particular school; but, although I note with interest that there are both free hand and mechanical drawings exposed here as the result of the year's course, permit me to narrow my remarks to the subject of mechanical drawing, concerning which I know the most and in which I am most interested.

"If printing be described as the art preservative of arts, drawing might with equal aptness be called the art definitive of arts. It is to the mechanical arts what notation is to music, algebra to mathematics. In its wonderful manifold qualities it is used for illustration, description, expression; for record and study; as an aid to invention, design, calculation, tabulation and generalization.

"As a means of illustration it stands unrivaled, conveying ideas in sequence, and logical demonstration where words would be lame or useless. Without it the college professor or the Franklin Institute lecturer would be lost. That it excels words in the matter of description is amply proved by its free use in descriptive circulars of machines and by its entry into columns of daily papers. As a means of record, our own Patent Office has recently recognized it as far superior to models, which have in most cases been dispensed with. It has this advantage over models that, while a model may be tampered with and show no signs of change, a drawing would, even if there were but one copy extant, be less easily altered; if altered it shows the fraud more plainly; and, besides this, there are likely to be duplicates or copies of the original drawing, which will attest any change or additions made in after years by any one to add to or detract from the value of the invention. As a means of study, who could learn anything about the slide valve, for instance, without diagrams? A few pencil lines relieve the imagination of the great strain necessary to picture the actual or relative positions of the moving and fixed parts. As an aid to invention, there are many who will certify how the friendly line has saved thought, time and expense, has preceded and helped along the birth of new movements and combinations, and revealed faults which would have been very expensive to remedy in the pattern, model, or actual machine. What is true of original invention is true of design for well-known devices.

"Of course, knowing the value of drawing, it is easy to see that it

should be of the right sort. A mechanical drawing should be accurate, reliable, neat, universally understood. If it is not accurate in its lines and reliable as to its dimensions, it may be the source of great expense and trouble. If it be not universally understood, it will be of little value. It must be self-explanatory. Chordal tells of a young man who prepared a drawing to send to a distant city, and with it there were several pages of fools-cap explanations. Being asked what the fools-cap was for, he replied, 'to explain the drawings;' and on then being asked what the drawings were for, he said they were to assist the explanations!

"There is much instruction given in drawing that is worse than useless. The learner gets tricks that are hard to unlearn, and cause him and others trouble. I remember my own course in mechanical drawing, in one of the most noted schools in this country. I had gone all through projection, perspective, etc., had drawn highly colored copies from expensive French drawings, and my crowning effort was a canal lock of most complicated structure, having upon its beautiful surface elevation, plan and section, in all the glories of cut stone, brick, cast and wrought iron, and brass, with impossible water in the foreground and in the dim distance. Its winding steps were my night-mare until I had finished them, and my pride and hope when the last dot of gamboge was put upon the knobs on the railings. Having completed such a magnificent work of art, such a triumph of engineering, there were no more worlds to conquer. I was ready to tackle anything, from a taper key to a Bessemer steel works. But, alas! When I entered the works, expecting to be set right to work on compound marine engines, I was put to tracing stub ends; and when allowed to trace a fly-wheel my school compasses reamed out the centre, and then the bow pen cut the middle circle clean out, so that my chief never found out about the reaming.

"I had never been taught to draw all the small circles first, to draw circles before the straight lines which were tangent to them, nor any of the hundred similar little things which should have preceded the magnificent masonry of the hydraulic lock. In a properly managed drawing class this would have been impossible. Let me say, in conclusion, that the art of drawing, and its practice, will save to him who possesses it, and to others, time, money, material, labor and annoyance."

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ON THE EFFECT OF PROLONGED STRESS UPON THE STRENGTH AND ELASTICITY OF PINE TIMBER.

By Prof. R. H. Thurston.

Presented to the American Association for Advancement of Science, Cincinnati Meeting, August, 1881.

In papers read before the American Society of Civil Engineers at various dates,* the writer has given the results of investigations made to determine the behavior of metals under loads of varying magnitude and under intermitted stresses, and to ascertain in what cases and under what conditions the variation, with period of stress, of the normal line of elastic limits, discovered and announced by him in the year 1873, occurs in practice.

Experiments made by Mr. Herman Haupt,† forty years ago, revealed a fact not even now generally understood and appreciated—that *timber* may be injured by a prolonged stress far within that which leaves the material uninjured when the test is made in the usual way and occupies a few minutes only.

Thus, using pieces $60 \times 3 \times 1$ inches ($152{\cdot}4 \times 7{\cdot}62 \times 2{\cdot}54$ cm.) set as-

* Trans. Am. Soc. C. E., 1873–80; Jour. Franklin Inst., etc., etc.

† Haupt on Bridge Construction.

cantilevers with a breaking moment, due the load, of $P\,l = 48P$ inch-pounds ($122P_m$ kilog.-metres) he obtained for the value of $R = \frac{6\,w\,l}{b\,d^2}$ the following figures:

Kind of Wood.	R.	Time.	Remark.
White pine,	2272	10 minutes.	Injured.
" "	1548	16 days.	"
Hemlock,	2624	5 minutes.	"
"	1620	16 days.	"
Yellow pine,	2848	5 minutes.	"
" "	1800	16 days.	"
Locust,	5504	2 minutes.	Not injured.
"	3600	$3\frac{1}{2}$ days.	Injured.
"	2304	16 days.	"
White oak,	4248	15 minutes.	Not injured.
" "	7200	15 minutes.	Injured.
" "	3648	40 hours.	Not injured.
" "	4088	48 hours.	Injured.

All samples tested were considered good selected timber.

An extended series of experiments made intermittently in the mechanical laboratory of the Stevens Institute of Technology, Department of Engineering, during some years past,* had included an examination of this subject and the result has confirmed Haupt's earlier work and has given a tolerably good idea of the effect of prolonged stress in modifying the primitive relation of stress and strain where the wood is good Southern yellow pine.

A selected yellow pine plank was obtained for test, the history of which was known. The stick was cut at Jacksonville, Florida, in October, 1879, was received early in the following year and was piled in the yard, air-seasoning, until taken for test in the spring of 1880. The plank measured $4'' \times 12'' \times 24'$ ($10{\cdot}16 \times 30{\cdot}48 \times 731{\cdot}52$ cm.). When tested, it had been seasoning six months, the latter part of the time indoors.

From the middle of this plank a stick was first cut $3'' \times 3'' \times 24'$ ($7{\cdot}62 \times 7{\cdot}62 \times 731{\cdot}5$ cm.) and from this was cut a set of ten pieces from 40″ to 54″ long (101·6 to 137·2 cm.) and from $1\frac{1}{4}''$ to 3″ square in cross-section (3·16 to 7·62 cm.) square. These latter pieces were

* Trans. Am. Assoc. for Advancement of Science, 1879–1880; Journal Franklin Inst., Oct., 1879, Sept., 1880.

tested on various conditions, as then reported,* to determine the values of their moduli of elasticity and of rupture.

The moduli of rupture were usually 11,000 to 12,000 for the expression $R = \frac{3}{2} \frac{P\,l}{b\,d^2}$ (in metric measure, 773·3 to 843·6) and the moduli of elasticity ranged from two to two and a quarter millions (in metric measure, $10^6 \times 1406$ to 158175×10^4). In specific gravity the wood ranged from 0·75 to 1·00, usually about 0·85. When kiln-dried to a moderate extent, the density was but little altered, if at all, but the modulus of elasticity rose to two and a half millions (17375×10^5) and the modulus of rupture was increased about 20 per cent.

From the previously unused part of the plank a set of three test pieces was cut about 1 inch (2·54 cm.) square in section and tested on supports 40 inches (101·6 cm.) apart, to determine their breaking loads. The result is shown in detail in the appended table. In these specimens the annual rings were in the cross-section of each piece, indicated by lines making angles of 45° with the edges. These pieces broke at 345, 380 and 410 pounds respectively. The weakest piece broke by splintering, and had it been as sound as the others would probably also have sustained a somewhat heavier load. As will be seen by comparison with the others and with subsequent tests, the deflection of the strongest piece in the set is exceptionally small and the piece probably exceptionally strong and stiff. We may therefore take 375 pounds (170 kilog.), or a trifle over, as a good average for loads breaking pieces of this size.

Nine other pieces were cut and dressed to the same size and were mounted on supports 40″ apart, in a frame arranged for the purpose in the workshop of the Institute, in three sets of three each.

These sets were loaded thus:

1st set,	250 pounds (113·6 kilogs.);	Table 2.
2d set,	300 pounds (136·4 kilogs.);	Table 3.
3d set,	350 pounds (158·1 kilogs.);	Table 4.

Or to about 60, 80 and 95 per cent. of their probable maximum strength, as indicated by ordinary test of the companion lot above described. Their deflections were measured when set, and at intervals subsequently, by means of an accurate micrometer reading to ten-thousandths of an inch.

* Trans. Am. Assoc., 1880; Journal Franklin Inst., 1880.

The whole set of bars, loaded most heavily as above, broke within two days; one bar yielding, as shown in Table 2, at the end of a period included between observations taken at $4\frac{1}{2}$ and $13\frac{1}{2}$ hours from the beginning, the second breaking at some time between 27 and $30\frac{1}{2}$ hours and the third giving way at the end of 43 hours. A load of $87\frac{1}{2}$ per cent., the maximum obtained by usual methods of test, is thus shown to be capable of breaking the piece under the conditions here described, and an apparent "factor of safety" of $1\frac{1}{7}$th is evidently not a factor of safety at all when time is given for the piece to yield.

The second set, loaded with 0·75 the maximum momentary weight, all broke, as is shown by Table 3, one at the end of about $3\frac{1}{2}$ days, another after 5 days, and the third at the end of a little more than a month. It is probable that these differences of time are due to differences of strength more than to variations of the effect of time of stress. A "factor of safety" of $1\frac{1}{3}$ is evidently not a real factor of safety for wood in such cases as this.

The behavior of the third and last set of test pieces is shown in Table 4. These pieces were loaded with 60 per cent. of the average breaking weight under ordinary test. Left under this load, the deflection, in every instance, slowly and steadily increased from about one inch (2·54 cm.) to some considerably larger amount at the end of the period of investigation. Fortunately, as is indicated by a comparison of these initial deflections with those observed under the same weights when testing the first set, and by their close accordance with each other, these pieces were all good samples of a good quality of yellow pine.

The increase of deflection was almost precisely the same for all for several months, a fact which is of importance, as showing not only the gradual progress and the steadiness of yielding, but also that no accident produced final rupture. Finally, after several months (about 6000 hours; the exact time is uncertain), the piece which had at the beginning shown most pliability broke completely down. The next piece to break was that which was intermediate in stiffness between the two others; it broke at the end of about 9000 hours—precisely one year from the date on which the load was imposed.

The last of the three pieces of this set still carried its load of 60 per cent. of the maximum under ordinary test at the last date, but it was still very slowly but unmistakeably yielding, its deflection having increased nearly 0·4 inch (1·016 cm.) during the preceding five

months. *It finally broke July 31st, 1881, about eleven thousand one hundred hours after it received its load* (15 *months*), which load was, it will be noted, but about 60 per cent. of its estimated—and probably practically correct—original breaking weight.

This very remarkable result fittingly terminated this series of tests of wood subjected to prolonged stress. An inspection of the broken bars gave no indication of reduction of strength by decay; the pieces were perfectly sound and the fractures showed excellent material.

Comparing the ultimate deflections attained by the several sets of bars it is seen that the average under ordinary test was about 1·8 inches (4·6 cm.). Under a load 0·95 that then carried, the rods broke at a deflection of 2·4 inches (6 cm.); loaded to ·80 the maximum, the deflection became, at the end, 3 inches (7·62 cm.) as a maximum, and the ultimate deflection of the most lightly loaded pieces (70 per cent., the maximum load) was something less.

The last set being compared with the first, it is seen that a load of 60 per cent., the maximum given by the usual form of test, is for such pieces unsafe, and that one-half the ultimate deflection under usual methods of test marked a point beyond which loads become certainly unsafe, although it would seem that a slightly smaller load might have been carried indefinitely, or until decay should weaken the timber. A factor of safety of two would possibly have permitted indefinite endurance under static load.

Taking the probable breaking load under unintermitted stress as 50 per cent., that sustained as a maximum under usual tests, and *then* applying a factor of safety of two, we obtain as a safe factor, based on the ordinary test, 4.

The writer would conclude, then, that timber may be placed with the "tin-class" among metals, as exhibiting a depression of the normal series of elastic limits under prolonged stress, and that this effect is so serious in its character and so important in its effects that an extended and complete investigation of the phenomena as exhibited in timber of various sizes, and of all the kinds in use in engineering, or in construction generally, would be of great value, even if not imperatively demanded.

In brief, the conclusions to be drawn from the research here described as having been made during the past fifteen months in the Mechanical Laboratory of the Department of Engineering of the Stevens Institute of Technology are evidently that small sections of

yellow pine timber yield steadily over long periods of time under loads exceeding 60 per cent., the maximum obtained by ordinary tests of their transverse strength, and finally break after a period, which with the lighter loads may exceed a year; that deflections half the maximum reached under test may be unsafe for long periods of time, and that a factor of safety of at least 4 should be used for permanent static loads when the character of the material is known.

The writer would, in the light of what is now known, always use a factor of safety of at least 5 under absolutely static load, and when the uncertainties of ordinary practice as to the exact character of material, and especially where shake and the impact of live load were to be considered, would make the factor not less than 8, and for much of our ordinary work 10.

The above experiments were arranged and supervised by Mr. J. E. Denton, and the observations made and recorded by Mr. A. Riesenberger, to both of whom the writer is under great obligations for intelligent and zealous assistance and co-operation in this as in many other investigations.

Hoboken, N. J., July 31st, 1881.

TABLE 2.

Time test; $P = 350$ lbs.

A. $b = 1·1$; $d = 1·1$ — B. $b = 1·12$; $d = 1·12$ — C. $b = 1·1$; $d = 1·1$

Distance between supports 40″.

Load Lbs.	A. Time load was applied. Hours.	A. Deflection. Inches.	B. Time load was applied. Hours.	B. Deflection. Inches.	C. Time load was applied. Hours.	C. Deflection. Inches.
40 (weight of box).						
350	....	·1565	....	·1705	....	·1840
350	....	1·7350	....	1·7175	....	2·0300
350	.18	2·3385	Between 27 and $30\frac{1}{2}$	Broke	$\frac{1}{4}$	2·3500
350	43	Broke			Between $4\frac{1}{2}$ & $13\frac{1}{2}$	Broke

TABLE I.

Usual method of test.

A.		B.		C.	
$b = 1·113$; $d = 1·105$		$b = 1·107$; $d = 1·107$		$b = 1·1$; $d = 1·1$	

Distance between supports 40″.

Load, lbs.	Deflection, inches.	Load, lbs.	Deflection, inches.	Load, lbs.	Deflection, inches.
0	—	0	—	0	—
50	·2127	50	·2035	50	·2188
After 5 min.	·2164	After 5 min.	·2125	After 5 min.	·2231
100	·4339	100	·3935	100	·4528
" 5 "	·4378	" 5 "	·4000	" 5 "	·4623
150	·6575	150	·5805	150	·6833
" 5 "	·6696	" 5 "	·5835	" 5 "	·6950
200	·8984	200	·7640	200	·9298
" 5 "	·9146	" 5 "	·7730	" 5 "	·9468
250	1·1552	250	·9630	250	1·2058
" 5 "	1·2286	" 5 "	·9775	" 5 "	1·2433
300	1·5109	300	1·1805	300	1·5648
" 5 "	1·5931	" 5 "	1·2180	" 5 "	1·6713
" 6 "	1·6029	350	1·4705	" 6 "	1·6883
350	1·9329	" 5 "	1·5381	325	1·8568
380	Broke	" 6 "	1·5700	340	Splintered
....	....	410	Broke	345	Broke

Table 3.

Time test; $P = 300$.

A.	B.	C.
$b = 1·11$; $d = 1·08$	$b = 1·1$; $d = 1·12$	$b = 1·1$; $d = 1·12$

Distance between supports 40″.

Load lbs.	Time load was applied. Hours.	Deflection. inches.	Time load was applied. Hours.	Deflection. inches.	Time load was applied. Hours.	Deflection. inches.
40 (weight of box).						
300	....	·1641	....	·1470	....	·1930
300	....	1·4461	....	1·1215	....	1·6725
300	1	1·5980	½	1·1835	1	1·8660
300	3	1·6666	3	1·2370	2	1·8950
300	5	1·7316	4¼	1·2735	18½	2·2450
300	22½	1·9171	5	1·2770	44	2·5450
300	47½	2·1026	21¾	1·4280	50	2·5920
300	54	2·1316	46¾	1·5485	66½	2·7310
300	70½	2·2596	69¾	1·6380	74½	3·0000
300	78½	2·4796	77¾	1·7740	betw'n 79½ & 88½	broke.
300	95½	2·7586	94¾	1·8505	....	....
300	118½	3·0586	117¾	1·9360	....	....
300	121	broke.	141¾	1·9610	....	....
300	....	....	165¾	1·9830	....	....
300	....	....	189¾	2·0050	....	....
300	....	....	238	2·0280	....	....
300	....	....	262	2·0440	....	....
300	....	....	286	2·0580	....	....
300	....	....	310	2·0630	....	....
300	....	....	335	2·0810	....	....
300	....	....	359	2·1110	....	....
300	....	....	406	2·1310	....	....
300	....	....	430	2·2110	....	....
300	....	....	454	2·2820	....	....
300	....	....	478	2·4570	....	....
300	....	....	502	2·5510	....	....
300	....	....	526	2·5870	....	....
300	....	....	598	2·6435	....	....
300	....	....	622	2·6520	....	....
300	....	....	646	2·6600	....	....
300	....	....	919	broke.	....	....

TABLE 4.

Time test: P = 250 lbs.

No. 1 — $b = 1·08''$; $d = 1·1''$ | No. 2 — $b = 1·08''$; $d = 1·1''$ | No. 3 — $b = 1·1''$; $d = 1·1''$

Distance between supports 40''.

Load. lbs.	No. 1 Time load was applied. Hours.	No. 1 Deflection, inches.	No. 2 Time load was applied. Hours.	No. 2 Deflection, inches.	No. 3 Time load was applied. Hours.	No. 3 Deflection, inches.
57 (weight of box)						
250	....	·1342	....	·1532	....	·1290
250	....	1·0317	....	1·0403	....	·9821
250	91	1·2927	90	1·3757	89	1·2696
250	161½	1·3967	160½	1·5132	159½	1·3796
250	185½	1·4077	184½	1·5402	183½	1·4076
250	210½	1·4237	209½	1·5592	208½	1·4246
250	233½	1·4372	232½	1·5862	231½	1·4446
250	258½	1·4942	257½	1·6602	256½	1·5196
250	281½	1·5227	280½	1·7042	279½	1·5651
250	305½	1·5377	304½	1·7217	303½	1·5736
250	329½	1·5547	328½	1·7402	327½	1·5856
250	353½	1·5657	352½	1·7492	351½	1·6036
250	402	1·5797	401	1·7662	400	1·6216
250	426	1·5897	425	1·7762	424	1·6286
250	450	1·5957	449	1·7852	448	1·6346
250	474	1·5997	473	1·7872	472	1·6396
250	499	1·6097	498	1·8032	497	1·6506
250	523	1·6257	522	1·8132	521	1·6636
250	570	1·6617	569	1·8632	568	1·6916
250	594	1·7047	593	1·8832	592	1·7336
250	618	1·7417	617	1·9522	616	1·7646
250	642	1·7777	641	1·9982	640	1·8066
250	666	1·8187	665	2·0342	664	1·8456
250	690	1·8347	689	2·0522	688	1·8566
250	762	1·8677	761	2·0852	760	1·8866
250	786	1·8777	785	2·0932	784	1·8946
250	810	1·8827	809	2·1032	808	1·9026
250	1195	1·9832	1194	2·1822	1193	1·9906
250	2107	2·1777	2106	2·3912	2105	2·1696
250	2923	2·2757	2922	2·4917	2921	2·2676
250	6715	2·9297	6066	broke.	6713	2·6416
250	8899	broke.	....	....	....	....
250	....	....	....	....	11100	broke.

AN EXPERIMENTAL INQUIRY INTO THE RELATIVE ECONOMIC EFFICIENCIES OF A CORLISS CONDENSING AND A CORLISS NON-CONDENSING ENGINE, WORKED BY SATURATED STEAM OF NEARLY THE SAME BOILER PRESSURE IN UNJACKETED CYLINDERS.

By Chief-Engineer ISHERWOOD, U. S. Navy.

An important problem in steam engineering is the determination of the boiler pressure at which the economic efficiency of a non-condensing engine becomes equal to that of a condensing engine using steam of the same boiler pressure; there being included among the factors of the problem the difference in the temperature of the feed-water in the two cases, and the power required to work the air-pump of the condensing engine.

In favor of the condensing engine are the very much less back pressure against the piston than with the non-condensing engine, and the greater measure of expansion with which the steam can be used in consequence of that less back pressure. And in favor of the non-condensing engine are the higher temperature of the feed-water than with the condensing engine, and the saving of the power required to work the air-pump.

Now, as the back pressure in the two cases may be taken as practically constant, say, for good practice, 3·5 pounds per square inch against the piston of the condensing engine, and 16 pounds per square inch against the piston of the non-condensing engine; and, as the temperature of the feed-water in the two cases may also be taken as practically constant, say 100 degrees Fahrenheit for the condensing engine and 200 degrees for the non-condensing, there is evidently a boiler pressure—or rather an initial cylinder pressure—at which the economic efficiencies of the two types of engine become equal.

From the data it would seem that the boiler pressure in question admitted of exact calculation and did not require an experimental determination, and such would be the fact were the steam used in cylinders whose material remained unaffected by heat, but as the cast iron of cylinders is greatly affected by heat, accepting and deliv-

ering it largely and with wonderful rapidity, the question passes from the abstract to the concrete form, becoming purely experimental, like all questions in physics, no confidence being due to any merely mathematical discussion of the case. One of the most important causes of steam condensation in a cylinder being the difference between the extreme temperatures in it during a double stroke of the piston, and these extremes, with constant initial pressure, being greatly affected by the back pressure which lessens them as it increases, this latter factor influences the problem to a far greater extent than its statical value alone. The extent of that influence can only be ascertained by experiment which including the obscure physics of the subject, unknown to calculation but potent on the result, is able to give a true solution when a mere mathematical treatment would lead to excessive error.

As experiments have abundantly proved that no economic gain is to be obtained by using steam with measures of expansion beyond the very moderate ones easily commanded in non-condensing engines worked by steam of not less than 70 pounds boiler pressure per square inch above the atmosphere, the idea may be definitely dismissed that the possibility of carrying expansion in the condensing engine to a greater degree than in the non-condensing one, is an economic advantage.

The higher temperature of the feed-water with the non-condensing engine results from the greater sensible heat of its exhaust steam which is utilized by means of a "heater" in raising the temperature of the feed-water. The sensible heat thus utilizable with either engine is practically controlled by the back pressure against which the steam exhausts; the higher that pressure, therefore, the higher will be the temperature of the feed-water; and as the non-condensing engine exhausts against a much higher pressure than the condensing one, the temperature of its feed-water will be correspondingly higher.

The same boiler furnishing equal weights of steam of the same pressure in equal times, but supplied with feed-water of different temperatures, will have a higher economic vaporization with the hotter feed-water, additional to what is due to the difference of heat to be imparted in the boiler in the two cases, owing to the slower combustion of the fuel. The hotter the feed-water, the less heat is required to be imparted to it in the boiler, so that for equal weights vaporized in equal times, the slower proportionally will be the rate at which the fuel is consumed, whence results that a less quantity of heat being

thrown on the heating surface of the boiler in a given time, the more of it will be absorbed. This gain would appear were the economic results of the experiments to be measured by the weight of fuel consumed per hour per horse-power developed, but it does not appear when they are measured by the number of Fahrenheit units of heat consumed per hour per horse-power.

To work an air-pump requires a power equal to the lifting of the water of condensation and the condensing water a certain height, to the expulsion of the air and uncondensed vapor, and to overcoming the friction of the mechanism, all of which is saved by the non-condensing engine.

Comparable experiments with the two types of engine are rarely found, but being in possession of the details of the trials made in 1878 on a Corliss condensing steam engine in the city of Mulhouse, Alsace, Germany, and published in the last May and June numbers of this journal, and of the still more copious details of the excellently conducted trial made by Mr. John W. Hill of a non-condensing Corliss steam engine, in 1874, for the Fifth Cincinnati Industrial Exposition, I am able to offer an experimental determination of the boiler pressure which in the case of the non-condensing engine gave an economic result equal to that given by the condensing engine operated with nearly the same boiler pressure.

Both engines were land engines, both had precisely the same valves and valve gear, both had horizontal cylinders with the same stroke of piston, both had the same proportion of space in clearance and steam passage at one end of the cylinder to space displacement of the piston per stroke; the only dimension of importance in which they differed was the diameter of the cylinder, which was fifty per centum greater in the condensing engine than in the non-condensing. In both engines saturated steam was used without cushioning or sensible release or lead. Neither was steam-jacketed; the cylinder of the non-condensing engine had no jackets, and although that of the condensing engine had jackets, yet during the experiments with it selected for comparison (Experiments *B* and *C* of the table opposite page 372 of the last May number of this journal) there was no steam in them. Both cylinders were well felted and lagged.

The valves and valve gear of both engines were alike. Each cylinder had two steam valves, one at each end and upon its upper side; and two exhaust valves, one at each end and upon its lower side.

The valves were all horizontal circular slides working with the steam pressure on their backs. They were operated by bell-crank levers keyed to their stems, to which levers rods were articulated connecting them with a wrist-plate oscillating on a journal supported by the side of the cylinder. The wrist-plate received its motion from a rod hooked on its arm at one end and attached at the other to an upright lever at the side of the engine frame, which in turn received its motion from the rod of an ordinary eccentric. The wrist-plate is moved through an arc of considerable extent, and owing to the manner of its connection with the eccentric, has its speed of oscillation maintained after the crank has passed its dead centres; so that approximately the initial opening and final closing of the valves are performed while they are at their greatest speed. The valves when closed have a slow movement, because their connecting rods are then in a position approximately radial to the journal on which the wrist-plate oscillates. The two rods connecting with the exhaust valves have permanent articulation with the levers on their valve stems; but the two rods connecting with the steam valves are detachable from the levers of their valve stems, the detachment being effected by the action of the governor at any point during the stroke of the piston. The steam valves, when detached, being free, are closed quickly by the fall of a weight suspended by a rod from the bell-crank on the valve stem, and working in a dash-pot beneath. This detachment and quick closing of the steam valves enables them to be used also as cut-off or expansion valves, the point of cutting off being variable by the governor. The two rods connecting the wrist-plate with the levers of the steam valves are attached to these levers by a hook on the upper side of the lower branch of what is called a "crab claw" which is jointed to the rod, and the detachment of this hook from the valve lever is effected by a cam-like projection or stop which oscillates on the valve stem bushing and is connected with the governor. The position of this stop being thus variable by the vertical movement of the governor, causes the detachment of the hook earlier or later in the stroke of the piston accordingly, and thus changes the point of cutting off the steam.

All the valve stems extend clear through the backs of the cylindrical valves, for which distance they are made flat, the backs of the valves being slotted to receive them. Thus the valves are not sus-

pended on their stems, which latter only work but do not support them, leaving the valves free to rest directly on their seats and follow down the wear.

The purpose of this valve gear is to open and close the valves with the maximum velocity. To cut off the steam by detaching the steam valves and leaving them free to close with as great velocity as can be given to them by a falling weight, the moment of detachment being variable by the action of an ordinary governor. The economic effect to be obtained being whatever might be due to the lessening of the small rounded corner on the indicator diagram where the cut-off valve closes, formed by the necessarily slow or gradual closing of that valve in any case. And also to the quick opening and closing of the exhaust valve, which allows the minimum back pressure against the piston to be obtained as quickly as possible and held as long as possible. The valve gear was also used to graduate the power, instead of a throttle valve, the graduation being effected with a constant initial cylinder pressure by the shorter or longer cutting off of the steam. And as this valve gear was contrived in the faith that every increase of expansion with which the steam was used, down to nearly the back pressure, increased its economic efficiency, the supposition was that the shorter points of cutting off, following each decrease in the load, caused a material saving of fuel. Further, the use of four valves, two at each end of the cylinder and of the slide type, reduced the space in the steam passages to the minimum, and thereby undoubtedly saved fuel to a corresponding extent; while the straightness and shortness of these passages prevented the lowering of the steam pressure and the raising of the back pressure due to longer and more tortuous passages.

Both the condensing and the non-condensing engines had been put in the best state possible for the trials, which were made with the utmost care and every precaution for exactness by competent and disinterested persons. In both cases indicator diagrams were taken every fifteen minutes from each end of the cylinders, together with a complete set of observations of the other data. The steam pressure, load, point of cutting the steam, speed of piston and all the other conditions were maintained without sensible variation during each experiment. All the instruments and measures used were previously tested. The revolutions of the engine shaft were noted from a counter. The weight of feed-water consumed was accurately measured in tanks and

there was no doubt that all this water was vaporized and that all the steam generated from it entered the cylinders whose pistons and valves had been secured steam tight. In both experiments, the power required to work the engines, *per se*, was calculated from indicator diagrams taken from the unloaded engine with its piston at the same speed as during the experiment.

Had the cylinders in the two cases been of the same diameter, and had their piston speeds been the same, the experiments would have been absolutely comparable. As it was, the non-condensing cylinder had a diameter of $16\frac{1}{16}$ inches, while the diameter of the condensing cylinder was 24 inches, which was in favor of the latter, economically, as regards the loss by cylinder condensation, for that loss, other things equal, is less with greater diameters of cylinder. The speed of the piston of the non-condensing engine was, however, greater than that of the condensing engine in the proportion, roundly, of 60 to 49, which was in favor of the former, economically, as regards the loss by cylinder condensation, for that loss, other things equal, is less with greater speeds of piston. These two differences, therefore, opposing each other as regards the same kind of loss, may be taken to neutralize each other, if not wholly, at least in great part.

In the following table will be found the data and results of these experiments. The quantities have been grouped for facility of reference, and they are so completely described on their respective lines that no further explanation is required. By the indicated horses-power is meant the power calculated for the pressure representing the mean ordinate of the indicator diagram. The net horses-power is the power calculated for what remains of the indicated pressure after deducting the pressure required to work the unloaded engine. The total horses-power is calculated for the sum of the indicated pressure on and the back pressure against the piston. The net horses-power is the only power which is commercially valuable, or applicable to external work.

The temperature of the feed-water given in the table is not the experimental temperature which, in both cases, was considerably less, owing to the water being delivered into tanks for measurement, where it rapidly cooled. The tabular temperature is what it would have had, had it been pumped directly into the boiler without passing through the measuring tanks; and the number of Fahrenheit units of

heat imparted to it, as given in the table, is the number that would have been imparted had it had the tabular temperature.

The quantities in the table are the means of all the indicator diagrams and of all the observations taken. Those for the condensing engine are from the two experiments made on it by a committee for the Industrial Society of Mulhouse, and are the means in function of the duration of each experiment.

Table Containing the Data and Results of Experiments made on Two CORLISS *Steam Engines—one Condensing, the other Non-condensing—to Determine their Economic Efficiency. In both cases Saturated Steam was used without Steam-jacketing, and there was no Cushioning in the Cylinders.*

		CONDENSING ENGINE. Mean of two experiments made by a Committee for the Industrial Society of Mulhouse in Alsace, Germany.	NON-CONDENSING ENGINE. Experiment made by John W. Hill for the Fifth Cincinnati Industrial Exposition.
DIMENSIONS OF CYLINDER.	Number of cylinders,	1	1
	Diameter of cylinder, in inches,	24	16·0625
	Stroke of piston, in inches,	48	48
	Net area of piston, in square inches,	442·0698	201
	Space displacement of piston, in cubic feet, per stroke,	12·2797	5·5833
	Space in clearance and steam passage at one end of cylinder, in per centum of the space displacement of its piston per stroke,	2·4647	2·9158
	Date of experiment,	8th & 9th April, 1878	3d October, 1874.
	Duration of experiment in consecutive hours,	10·780 and 5·6772	8·0000
WEIGHT OF STEAM ACCOUNTED FOR BY THE INDICATOR.	Pounds of steam present per hour in the cylinder at the point of cutting off the steam, calculated from the pressure there,	....	1572·0734
	Pounds of steam present per hour in the cylinder at the end of the stroke of its piston, calculated from the pressure there,	....	1819·6034
	Pounds of steam condensed per hour in the cylinder to furnish the heat transmuted into the total horse-power developed by the expanding steam alone,	....	147·6858
	Sum of the two immediately preceding quantities,	....	1967·2892

		Condensing Engine. Mean of two experiments made by a Committee for the Industrial Society of Mulhouse in Alsace, Germany.	Non-condensing Engine. Experiment made by John W. Hill for the Fifth Cincinnati Industrial Exposition.
Engine.	Steam pressure in boiler, in pounds per square inch above atmosphere,	66·592	70·477
	Steam pressure in valve chests of cylinder, in pounds per square inch above atmosphere, . .	63·025	67·500
	Proportion of throttle-valve open,	Wide.	Wide.
	Fraction of stroke of piston completed when the steam was cut off, .	0·1050	0·2066
	Number of times the steam was expanded, . . .	7·9023	4·3657
	Pressure in the condenser, in pounds per square inch above zero, .	2·0747	....
	Pressure of the atmosphere, in pounds per square inch above zero, .	....	14·590
	Number of double strokes made per minute by the piston, . .	49·19785	60·10833
	Temperature of the feed-water, in degrees Fahrenheit, . .	100	200
	Number of Fahrenheit units of heat imparted to the feed-water, .	1109·3253	1009·5796
	Number of pounds of feed-water pumped into the boiler per hour,	3312·512	2186·870
Steam Pressures in Cylinder Per Indicator.	Steam pressure on piston, in pounds per square inch above zero, at the commencement of its stroke, .	70·9000	78·744
	Steam pressure on piston, in pounds per square inch above zero, at the point of cutting off the steam,	....	71·139
	Steam pressure on piston, in pounds per square inch above zero, at the end of the stroke of the piston,	....	17·533
	Mean back pressure against the piston, in pounds per square inch above zero, during the stroke of the piston,	5·1124	15·925
	Minimum back pressure against the piston, in pounds per square inch above zero, at commencement of stroke of piston, . .	....	15·000
	Mean indicated pressure on piston, in pounds per square inch, .	24·1451	25·3775
	Mean net pressure on piston, in pounds per square inch, . .	21·9810	23·4890
	Mean total pressure on piston, in pounds per square inch, .	27·2575	[illegible]
	Pressure on piston in pounds per square inch to work unloaded engine, . . .	2·1641	1·8885

		Condensing Engine. Mean of two experiments made by a Committee for the Industrial Society of Mulhouse in Alsace, Germany.	Non-condensing Engine. Experiment made by John W. Hill for the Fifth Cincinnati Industrial Exposition.
Horses-Power.	Indicated horses-power developed by the engine, . .	127·2987	74·3285
	Net horses-power developed by the engine, . . .	115·8939	68·7973
	Total horses-power developed by the engine, . . .	143·7140	116·6777
	Total horses-power developed by the expanding steam alone, .	....	55·7712
Economic Results.	Number of pounds of feed-water consumed per hour per indicated horse-power, . . .	26·0216	29·4217
	Number of pounds of feed-water consumed per hour per net horse-power, . . .	28·5823	31·7871
	Number of pounds of feed-water consumed per hour per total horse-power, . . .	23·0493	18·7428
	Number of Fahrenheit units of heat consumed per hour per indicated horse-power, ...	28866.4192	29703·5481
	Number of Fahrenheit units of heat consumed per hour per net horse-power, . . .	31707·0685	32091·6077
	Number of Fahrenheit units of heat consumed per hour per total horse-power, . . .	25569·1716	18922·3485
Cylinder Condensation.	Difference in pounds per hour between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator at the point of cutting off steam, .	....	614·7966
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator at the point of cutting off the steam,	....	28·1131
	Difference in pounds per hour between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator at the end of the stroke of the piston,	....	219·5808
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator at the end of the stroke of the piston, ...	....	10·0409

Remarks.

It is greatly to be regretted that, in the original report of the experiment with the condensing engine, the pressures at the point of cutting off the steam and at the end of the stroke of the piston were not given, for then calculations might have been made of its cylinder condensation similar to those made for the non-condensing engine, which would have revealed the cause of the equality of the economic commercial efficiencies of the two engines, notwithstanding the greatly less fraction utilized of the total pressure in the case of the non-condensing engine.

The total horse-power developed in the two cases represents the entire dynamic effect—useful and prejudicial—produced by the steam or fuel expended in equal times, and comparing the cost of the same in Fahrenheit units of heat, there appears that the total horse-power was obtained with the non-condensing engine for an hourly expenditure of 18922·3185 Fahrenheit units, and with the condensing engine for an hourly expenditure of 25569·1716 Fahrenheit units, the two costs comparing as 1·00000 to 1·35127, an enormous difference due to the greater cylinder condensation with the condensing engine combined with the greater number of units of heat required for the production of a pound weight of its steam owing to the less temperature of its feed-water. With the condensing engine there were required 9·8799 per centum more heat to vaporize a pound of feed-water than with the non-condensing engine; and diminishing the above 1·35127, there remains 1·21777 to 1·00000 for the ratio of the economic efficiencies of the non-condensing and condensing engines in function of the total horse-power, the whole of which difference was due to the less cylinder condensation in the non-condensing engine. Now with the non-condensing engine, the final cylinder condensation was 10·0409 per centum of the steam evaporated in the boiler, leaving 89·9591 per centum utilized in the production of power; dividing this latter quantity by the above 1·21777 there results 73·8720 per centum utilized in the condensing engine, which deducted from 100·0000 leaves 26·1280 per centum of the steam evaporated in the boiler condensed in the cylinder of the condensing engine. A condensation in the condensing cylinder of 26·1280 per centum of the steam evaporated in the boiler is then sufficient to account for the difference in the heat cost of the total horse-power in the two cases; and it is known from many experiments that this is about what occurs in an unjacketed

cylinder of the dimensions of the condensing engine cylinder using saturated steam with an expansion of nearly eight times.

By an indirect method, with the data obtained during the series of experiments made on the condensing engine by the committee of experts for the Industrial Society of Mulhouse, the condensation in the cylinder during experiments *B* and *C* hereinbefore referred to, was determined to be 29·6290 per centum of the steam evaporated in the boiler. (See page 435 of the last June number of this journal.)

One of the most important causes of that condensation is the difference of the extreme temperatures of the cylinder during a double stroke of the piston. In the condensing cylinder, the initial steam pressure being 70·9 pounds per square inch above zero, and the minimum back pressure, say, 3 pounds per square inch above the same, the temperatures corresponding to which are 303·62 degrees Fahrenheit and 141·67 degrees, this difference is 161·95 degrees. In the non-condensing cylinder, the initial steam pressure being 78·744 pounds per square inch above zero, and the minimum back pressure 15·9 pounds per square inch above the same, the temperatures corresponding to which are 310·76 degrees Fahrenheit and 215·10 degrees, this difference is only 95·66 degrees. And besides this cause of greater condensation in the condensing cylinder, there was the greater refrigeration produced by the greater measure of expansion with which the steam was used in that cylinder, than in the non-condensing cylinder.

The net horse-power, representing the portion of the total horse-power developed by the engine that was commercially useful, was obtained for the consumption of 31707·0685 Fahrenheit units of heat per hour with the condensing engine, and of 32091·6077 Fahrenheit units with the non-condensing engine; and if a very small allowance be made in favor of the latter for the greater economic vaporization in its boiler per pound of fuel, owing to the slower rate of combustion, the cost of the net horse-power in both cases will be equal; showing that a non-condensing engine with an unjacketed cylinder of the experimental dimensions, using saturated steam of $70\frac{1}{2}$ pounds boiler pressure per square inch above the atmosphere, with an expansion of nearly $4\frac{3}{8}$ times, gave the same commercial result—that is to say, the same net power for the same quantity of fuel per hour—as a condensing engine with a two and a quarter times more capacious unjacketed cylinder using saturated steam of $66\frac{1}{2}$ pounds boiler pres-

sure per square inch above the atmosphere with an expansion of nearly 8 times. Hence, under the experimental conditions, no economy would result from the employment of a condenser and air-pump, when the boiler pressure was not less than $70\frac{1}{2}$ pounds per square inch above the atmosphere. If the engine works with a variable load, this must be taken for the lower limit of pressure—not the average pressure—giving equality of economic effect.

Of the total pressure in pounds per square inch above zero with the condensing engine, there were utilized as net pressure $\left(\frac{21{\cdot}9810 \times 100}{27{\cdot}2575} = \right)$ 80·6420 per centum; and with the non-condensing engine $\left(\frac{23{\cdot}4890 \times 100}{39{\cdot}8365} = \right)$ 58·9365 per centum; the two comparing as $\left(\frac{80{\cdot}6420}{58{\cdot}9635} = \right)$ 1·36766 to 1·00000 or very nearly the 1·35127 to 1·00000 found as the ratio of the heat cost of the total horse-power in the two cases; so that the less fraction of the total horse-power utilized as net horse-power with the non-condensing engine just balanced the less heat cost of its total horse-power, enabling the net horse-power to be obtained for the same heat cost in both cases.

The correctness of these facts was confirmed some years ago at a large flour-making mill in New York City, which was operated by several non-condensing cylinders of moderate dimensions, unjacketed, and using saturated steam of about 90 pounds boiler pressure per square inch above the atmosphere, with a considerable measure of expansion. The proprietor was persuaded to add a surface condenser and an air-pump, and a variable cut-off, expanding the steam sufficiently more to retain the same mean cylinder pressure with the same boiler pressure, the expectation being that a marked difference in the weight of fuel consumed per hour to grind and dress the same number of bushels of wheat would result; and such indeed was the case, but in the opposite direction to the expectation, the power actually costing so much more fuel that the condenser and air-pump were removed and the original conditions restored.

The foregoing results are true for only the precise experimental conditions, and they will be modified by any of the causes which diminish cylinder condensation, as, for example, steam-jacketing the cylinders, super-heating the steam, employing larger cylinders, etc.,

for there is a greater economic gain possible by them for the condensing than for the non-condensing engine, as the former has the most cylinder condensation to be reduced. Consequently, therefore, just in proportion as the cylinder condensation is lessened by steam-jacketing, steam-superheating and larger cylinders, must the boiler pressure be increased for the non-condensing engine to sustain its equality of economic performance, gaining by the resulting increase of the fraction which the net power is of the total power, what it loses in decrease of relative cylinder condensation. It is probable, however, that with a boiler pressure of from 95 to 100 pounds per square inch above the atmosphere the non-condensing engine would give the net power with fully as much economy of fuel as the condensing engine using the same steam pressure with the measure of expansion found to produce the greatest economy, even with steam-jacketing, steam-superheating and cylinders of the largest dimensions in both cases.

For marine engines, the use of high pressure steam is important, because it lessens proportionally the dimensions of cylinders required for a given power, the dimensions for high powers having now become inconveniently large, and because it allows the removal of the air-pump and appendages. A surface condenser would still be required to furnish the boiler with distilled water, but the quantity of surface could be seriously reduced, the reduction being due not only to the fact that about one-tenth less heat is to be taken out of the exhaust steam, but that owing to the greater difference of temperature on the opposite sides of the condensing surface, a unit of this surface is proportionally more efficient for condensation.

Less than nine-tenths of the refrigerating water would also be used because the difference between its initial and final temperatures in the condenser would be very much greater, so that a portion of the power required for pumping this water with condensing air-pump engines would be saved.

The power expended in working the air-pump and the circulating-pump in marine engines is much greater than in land engines, because of the greater resistance against which they discharge, due to the height of the outboard column of water and to the greater tortuousness of the discharging pipes.

The advantages of the non air-pump engine are, of course, for the cases in which a *uniform* power is employed, as for merchant steamships. For naval steamships which are engined for the development

of high powers during short periods at long intervals, their principal steaming being done with very low powers, the condensing air-pump engine preserves its economic superiority.

DISCUSSION

Of the Papers of C. P. Sandberg on "Rail Specifications and Rail Inspection in Europe," of C. B. Dudley on the "Wearing Capacity of Steel Rails in Relation to their Chemical Composition and Physical Properties," and of A. L. Holley on "Rail Patterns," at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17th, 1881.

(Continued from page 118.)

C. E. STAFFORD, Steelton, Pa.: I must confess my high appreciation of Dr. Dudley's conscientious and painstaking work, and of his scientific methods in obtaining the data; but with his method of handling these results and with his conclusions drawn therefrom I cannot agree. The reasons for this difference of opinion I will endeavor to explain.

It is apparent on inspecting his Plates 6 and 7 that the majority of the slower-wearing rails are from the north track, and generally have a longer "time of service," a greater average tonnage per rail, and a smaller average tonnage per year per rail than the faster-wearing, the majority of which are from the south track, and generally have a shorter "time of service," a smaller average tonnage per rail, and a greater average tonnage per year per rail. Have these facts any significance? Have these differences of conditions to which they are subjected any bearing on the relative wearing capacity of these rails? I venture to say they have. I think, after a study of Table 1, (an arrangement of lines 17 and 18, Plate 8,) in connection with Plates 6 and 7, we will find that the slower wear of the 32 best rails is only partly due to qualities inherent in the rails themselves, but is principally due to external conditions favorable to slower wear.

In regard to the north and south track, we know that over the south track come the loaded cars from the West, and that over the

north track these cars return, most of them empty. It is evident that this means for the north track a less average tonnage per rail per year; or, in other words, a lower wheel-tonnage. When the load per wheel is less, the resistance and consequently the wear must, necessarily, be less, other things being equal.

"Time of service," also, has an important bearing on the question in hand. It has been only within the last five or six years, as Dr. Dudley has pointed out, that the roadbed of the Pennsylvania Railroad has reached its present admirable condition. Before this time the roadbed was more elastic, more yielding (and probably not uniformly so) than at present. These circumstances might cause a softer rail to be more durable than a harder one, owing to the fact that it would yield more or less to the bending force of the passing load and would thus get a bearing on each cross-tie. The harder rail, on the other hand, being stiffer and more unyielding, would not have this bearing uniformly, and would thus, to a greater or less degree, be subjected to the same conditions as a beam under shock, vibration and a rapidly moving load. Under such conditions, I believe, a harder rail would crush, break and perhaps wear out more easily and quickly than a softer rail. This agrees with Dr. Dudley's statement: "With the improvement in maintenance of way, during the last five or six years, the removal of rails from track from the first two of these causes has quite notably diminished." Under conditions as they now exist on the Pennsylvania Railroad, I believe, the harder rail will give the slower wear. With the ballast comparatively solid and unyielding, as at present, the rail, having a more nearly perfect and uniform bearing, and acting less the part of a beam than that of an anvil must, in my opinion, be a hard one to withstand the pounding of the locomotive and the abrasion due to combined rolling and sliding friction.

Viewed in this light, a hard or soft rail would be respectively preferable as the maintenance of way has become more or less practically perfect. Of the seven rails, in track seven years or less, included in the slower-wearing division, and whose phosphorus units average 40·95, there will be found but one showing, under the same conditions, a slower wear for the softer rail. As these rails were put in track during and after the improvement in maintenance of way they tend to confirm the proposition that with a well-ballasted track

the harder rails give the slower wear. Next under the head of "time of service" comes the consideration of the wheel-tonnage (the average tonnage per rail per year in Table I). This, as pointed out by Mr. Ashbel Welch, has been increased over 60 per cent. within the last five or six years. The speed and the weights of locomotives, cars and trains have also been increased within this period. With the increase of each of these quantities, resistance increases. This increased resistance must be overcome by increased friction between drivers and track, which, other conditions being the same, must result in greater or more rapid wear than formerly. Further, with greater speed and weights the defects in the rolling stock—as flat and improperly coned wheels, worn tires, etc.—must cause greater injury to the rail. It must be borne in mind that the average wheel tonnage of the north track is less than that of the south track during the entire period considered.

It will be noticed, in Table I, that the average tonnage per rail (not the average tonnage per rail per year) of the slower-wearing rails is much greater than that of the faster-wearing. I call attention to this fact because I believe it to be important. We know that the head of a rail when new is more or less rough, and that this roughness wears off rapidly with the first few million tons of service; consequently, if the loss is determined when the tonnage is low, the loss per million tons will not show the actual wearing rate during its future use. Of course, this influence on the wearing rate becomes less and less as the tonnage increases. After wearing off this roughness, it may be that the succeeding few million tons cold-roll or hammer the surface, causing it to more successfully resist subsequent wear. From the history of the road it is evident that those rails having a long time of service possess advantages in favor of slow wear. Not only have they a high tonnage, but they also have had a preparation and wear of the surface while the wheel tonnage was light; the later rails, have, on the other hand, been subjected from the start to a heavier wheel tonnage with the accompanying conditions unfavorable to slow wear. Upon the relative wear on different grades and curves, and combinations of the two, it is unnecessary to dwell.

Having tried to make plain the tendency of each of these conditions, I will now tabulate them:

Conditions Favorable to Slower Wear.	Conditions Unfavorable to Slower Wear.
North track.	South track.
Lighter wheel tonnage.	Heavier wheel tonnage.
Longer time of service.	Shorter time of service.
Greater tonnage.	Smaller tonnage.
Lighter grade.	Heavier grade.
Lower degree curve.	Higher degree curve.

I do not say these conditions will absolutely determine the relative positions of the 64 rails, because we do not know the ratio of the wear of a given rail under a known set of conditions (favorable or unfavorable, or both) to the wear of the same rail under another known set of conditions; also because of conditions not given in the data and because of exceptions named below. But what I have tried to make plain in the preceding remarks is that, in general, the 32 best rails have been in service under conditions, in the main, favorable to slower wear. When this is not the case, the rail, measured by phosphorus units, is hard, and with one exception (rail 915 referred to later), harder than its companion subjected, as far as known, to the same conditions.

To put it more concisely, the slower wear of the 32 best rails is due to external conditions, which, when summed up, are favorable to slower wear, and not to qualities inherent in the rails themselves; except in a few cases, and these when the rails are hard. If this statement is true, then Dr. Dudley's conclusion, that the slower wear of the 32 best rails is due to their being softer than the 32 faster wearing, does not hold.

With the object of learning further what averages of these 64 rails may point out to us, I have arranged them differently, as seen in Table II. We may thus be able to learn whether the indications of the first averages are confirmed or not; or whether the first averages, when studied with the second, may fairly be interpreted to point to, or at least not disprove, conclusions radically differing from those first drawn.

In Table I, which we have just been considering, Dr. Dudley has found that 32 rails of a certain average chemical composition show a much slower wear than 32 rails of a different average chemical composition. The 32 slower-wearing rails averaging softer than the 32 faster-wearing, the conclusion is drawn that this slower wear is due to this fact. Apparently, this inference is true, but as we have

just seen, this slower wear is probably due to other causes. In Table II we have a comparison of rails in the north and south track. This gives us hard and soft rails averaging practically the same chemically as those in Table I, but showing a decided difference in the wear per million tons, comparing the soft and hard of the one with the soft and hard of the other, respectively; also, in the ratio of wear between the soft and hard of each. In Table I this ratio is 1 to 2·03; in Table II, 1 to 1·16. Even this slight difference probably would not have appeared if the north and south rails had been equal in number in both curves and tangents. By consulting Dr. Dudley's Plates 6 and 7 the distribution will be found to be as follows: In tangents, 19 north track rails and 13 south; in curves, only 13 north track rails and 19 south. As it is, the south track rails with a wheel tonnage (average tonnage per rail per year) 62½ per cent. greater, there is a wear only 16 per cent. greater; and this with nearly equal average rail tonnage in both. With these circumstances in mind we may fairly conclude from Table II that, under the same conditions, the harder rails would give the better wear, which indication Table I does not contradict. This conclusion, like the one before, is in favor of the harder rails.

TABLE I.

	North track.	South track.	Total.	Average time of service.		Av. wear per million tons.	Average tonnage per rail.	Average ton per rail, per year.	C.	P.	Si.	Mn.	Phos. units.
All conditions slower wearing......	22	10	32	9y. —	5½m.	·0506	53,737,156	5,686,104	·334	·077	·060	·471	31·3
All conditions faster wearing.....	10	22	32	6	0	·1028	40,406,260	6,730,871	·390	·106	·047	·647	38·9

TABLE II.

	North track.	South track.	Total.	Average time of service.		Av. wear per million tons.	Average tonnage per rail.	Average ton per rail, per year.	C.	P.	Si.	Mn.	Phos. units.
N. track rails...	32	0	32	9	5¼	·0638	46,545,146	4,906,225	·329	·071	·064	·508	31·4
S. track rails...	0	32	32	5	11¾	·0740	47,285,770	7,978,227	·395	·111	·043	·631	38·9

We have considered the conditions furnished by Dr. Dudley favorable and unfavorable to slower wear. That there are other conditions which materially affect the relative wear of rails will readily occur to all.

Among others, in addition to those given, are the following: Whether the speed is the same over all of the rails; whether the rail is subjected to more than ordinary wear by the stopping and starting of, or by the decreasing or the increasing of the speed of trains, as at or near train and watering stations, switches, crossings, sidings, bridges, tunnels, grades, curves, etc.; whether on tangents both sides of track at the same place are at the same level; whether on curve, the rail is taken near entering tangent or elsewhere; whether the elevation of the outer rail in each case corresponds to the average speed of trains at that curve; whether the character and condition of ballast and roadbed is the same for all rails; and other conditions known to those familiar with maintenance of way.

These conditions, more or less local in their character have, it seems to me, been too little considered in the study of "the wearing capacity of steel rails with relation to their chemical composition and physical properties."

With these circumstances in mind, we must agree with Mr. Hunt that "averages are dangerous." In comparing different sets of rails, when, in each set, varying quantities (conditions) too indefinite to be averaged, or not averageable, are associated with others which are definite and can be averaged, and when the maximum and minimum in one set are greater or less respectively than the maximum and minimum of the other, and where a quantity in one rail differs greatly from the other rails in the same set, making the average to differ widely from any quantity of the same kind in that set, can averages give conclusions which can reasonably be accepted as true beyond a doubt? I do not think they can. We have seen that widely different conclusions *can* be drawn from indications given by averages made up from different arrangements of the same rails. Indeed, I believe that conclusions drawn from averages made up from any number of rails, under so many and such different conditions, would be of value only when confirmed by other data.

To properly study the relative wearing capacity of steel rails with reference to their chemical composition and physical properties, we must compare rails subject to the same conditions, as far as is

No. of group.	No. of rail	Time of service. Y.	M.	Track.	Grade.	Curve.	Tonnage.	Phos. units.	Loss per yard.	Loss per million tons.	Ratio of Loss. Harder rail 1
1	881	11	1	North.	92·4		55,546,811	59·2	3·85	·0693	1 : 1·50
	882	11	1	"				28·1	5·78	·1040	
2	883	10	2	"	95·04		52,174,969	35·7	2·54	·0487	1 : ·84
	884	10	2	"				32·7	2·13	·0408	
3	885	11		"	89·76		55,197,994	33·9	2·74	·0496	1 : 1·63
	886	11		"				27·9	4·48	·0811	
4	887	11		"	89·76		55,197,994	24·2	2·13	0·386	1 : 1·61
	888	11		"				21·3	3·44	·0623	
5	889	5	11	South.	21·13		44,620,100	33·1	3·54	·0793	1 : ·97
	890	5	11	"				41·7	3·64	·0816	
6	891	7	1	"	40·13		53,687,192	37·7	3·14	·0585	1 : 1·10
	892	7	1	"				33·7	3·46	·0644	
7	894	5	2	"	52·8		38,088,574	45·9	2·93	·0769	1 : 1·03
	895	5	2	"				45·5	3·03	·0796	1 : 1·04
	896	5	2	"				40·5	3·04	·0798	
8	913	9	4	North.			47,855,101	33·4	1·01	·0220	1 : 1·31
	914	9	4	"				26·9	1·32	·0288	
9	915	6	1	"			31,714,889	39·2	0·51	·0162	1 : [illegible]
	923	6	1	"				45·5	2·92	·0925	
10	916	6		"			31,127,829	37·5	1·42	·0456	1 : 7·12
	917	6		"				45·4	0·20	·0064	
11	918	10	6	"			51,720,011	29·9	0·71	·0137	1 : 0·71
	919	10	6	"				20·3	0·51	·0098	
12	921	5	1	"			27,622,230	37·9	3·05	·1104	1 : 1·50
	922	5	4	"				44·7	2·03	·0735	
13	924	4	1	South.			36,349,989	44·2	1·52	·0418	1 : 1·14
	925	4	1	"				43·8	1·73	·0476	
14	926	9	3	"			76,409,123	30·2	1·53	·0200	1 : 1·13
	927	9	3	"				28·1	1·73	·0226	
15	897	11	2	North (high side).	21·12	5°	52,370,617	33·2	7·31	·1396	1 : ·96
	898	11	2					28·1	6·99	·1335	
16	899	11	2	North (low side).	21·12	5°	52,370,617	22·3	2·24	·0428	1 : ·92
	900	11	2					32·2	2·44	·0466	

practical, and which only vary in their chemical composition and physical properties. I have attempted to do this in the foregoing table, made up from Dr. Dudley's level and grade tangents and grade curves. The members of each group have been in the same track the same time, are from the same locality, and have the same grade and curvature (if any); or, in other words, have been subjected to the same conditions, as far as known. It will be noticed that Nos. 893, 920 and 928 of the level and grade tangents have been omitted, and also all of level and grade curves excepting Nos. 897, 898, 899 and 900, because of the impossibility of grouping them in the

same manner, no two having the chemical composition and physical properties as the only variables. Phosphorus units have been taken to show the relative hardness.

Of these 16 groups, 10 decidedly indicate slower wear for the harder rail.

Of the remaining 6 groups, three (groups 2, 9 and 15) come well within the limits of error (·25 pound per yard) inherent in the calculation of the data as pointed out by Dr. Dudley; they cannot, therefore, be considered as exceptions. Groups 2 and 15 are but little outside of the limits of error. Rail 923 of group 9 is obviously abnormal. This is probably due to its being overheated, which the chemical analysis shows might easily be the case, and which the physical tests and its low specific gravity tend to confirm. We may say, then, that 13 of these 16 groups fairly indicate, if they do not definitely point out that, under the same conditions, the harder rail gives the slower wear.

Of course, these comparisons are too few to enable us to arrive at positive conclusions; but indications thus obtained are, I believe, far more valuable and trustworthy than those that averages would give us, made up from any number of rails under many different conditions.

Finally, it seems to me that the conclusions arrived at earlier in my remarks, together with and confirmed by the last, show strong evidence that, under the same conditions, the harder rail will give the slower wear.

O. Chanute, New York City: We are very much obliged, I am sure, to Mr. Sandberg for his paper upon "Rail Specifications and Rail Inspection in Europe." We have in the United States hitherto been inspecting rails somewhat haphazard, and we are glad to get the results of Mr. Sandberg's long experience.

We recognize that he was among the first, if not the very first to apply more rational and scientific rules to the designing of iron rails, and, more recently, to adapt these rules to the designing of steel rails, to conform to the capabilities and requirements of this new material. Although we have generally adopted, in this country, sections for steel rails which many of our railway men think even better than those of Mr. Sandberg, he is, nevertheless, the leader in whose footsteps we have followed ever since it has been established

that the fish-joint was the best method of fastening the rails together at the ends.

We have not, however, yet been able to formulate or to adopt any well-established relation between the height and weight of the rail and the weight and speed of the engines, such as he indicates in his Table No. 1. For instance, the New York Central rail is $4\frac{1}{2}$ inches deep, and weighs 65 pounds per yard, while its locomotives are of 37 tons maximum weight. The Erie rail is $4\frac{5}{16}$ inches deep, with 63 pounds weight per yard, and the Pennsylvania rail is $4\frac{1}{2}$ inches deep, with 67 pounds weight per yard, while the maximum weight of locomotives upon both these latter lines is 50 tons. Now, which is right? I am inclined to believe that the Pennsylvania Railroad follows the better practice, not because the other rails are too light for the engines, but because inasmuch as steel rails wear out by abrasion, and not by lamination, the Pennsylvania rail promises to be serviceable until about 12 pounds of metal per yard are worn off from the head, while the Erie rail will probably have to be removed from the track when some $8\frac{1}{2}$ pounds are worn off.

Timber is still so cheap with us that we have not hitherto concerned ourselves very greatly about the strength of our rails considered as beams. If after having adopted a rail section, say between 50 and 60 pounds, we have found it a little too limber under increasing weight of locomotives, we have simply put the ties nearer together, and we have thus arrived at the general practice of spacing them about two feet between centres, while I notice that Mr. Sandberg's calculations of required stiffness are based upon having the supports 3 feet apart.

We are careful, however, to limit the weight upon our driving-wheels to a maximum of 12,000 pounds (excepting a few experimental locomotives), and when our gradients and trains require more adhesion than can be obtained from the standard "American" engine, we think it better to adopt the "Mogul" type, with 6 drivers, or the "Consolidation," with 8 drivers; the latter having generally an average of but 11,000 pounds per driving-wheel, and being no harder on the rail than other classes of locomotives.

Believing that much of the wear of rails results from undue pressures, I have made some experiments to determine the area of the surfaces in contact between wheels and rails. These were obtained by jacking up a wheel, and introducing between it and the rail a piece

of thin tissue-paper, underlaid with a slip of black manifold copying-paper. Upon lowering the wheel, it generally crushes a hole in the paper, and gives a fair impression of the surfaces in contact. If the wheel and rail were inelastic, this contact would be a mere line, but as they both yield, it becomes a surface which varies with the weight on the wheel and with its condition.

I found that with 11,000 to 12,000 pounds weight upon a locomotive driving-wheel of about 5 feet diameter the pressures were generally 35,000 to 40,000 pounds to the square inch, although they occasionally ran up much beyond this, but that with 14,000 pounds on a driver the pressures became from 50,000 to 80,000 pounds to the inch, or beyond the elastic limit even of steel.

I also found that under empty freight cars, with say 2400 pounds on a 33-inch wheel, the pressures were generally 20,000 to 30,000 pounds per square inch; that with the car loaded with 11 tons, increasing the weight to say 5150 pounds per wheel, these pressures became about 35,000 pounds to the inch, while if the car was loaded with 20 tons, thus giving 7400 pounds per wheel, the pressures increased to 50,000 or 60,000 pounds to the square inch.

As we increase the weight upon our cars, therefore—and I believe this to be the correct and inevitable practice—we must be prepared to find our steel rails wear out faster than they hitherto have done. We may, perhaps, reduce the pressure by increasing the diameter of car-wheels, but my own judgment is that we should endeavor to limit the weight on locomotive drivers of 5 feet diameter to 12,000 pounds, and on 33-inch car-wheels to about 7000 pounds, so as not to bring crushing strains upon our rails and wheels.

I notice that Mr. Sandberg is disposed to think that our adoption of 30 feet as a normal rail length is an extreme limit. I believe, however, that our mills have found no difficulty in working up to this, and that we get only about 3 per cent. of shorter rails under the provision that not more than 10 per cent. may be delivered under 30 feet, down to 25 feet. The difficulties which he mentions as connected with ocean transportation of 30-feet rails need not concern us much at the present time. I believe that the iron rail mills of this country have the capacity for turning out about 1,000,000 of tons a year, if so many tons of iron rails were called for, and that the steel mills have a present capacity of about 1,500,000 tons, and a prospective capacity, by the end of this year, of some 1,750,000 tons of steel rails per annum.

Now this would furnish us enough rails, if fully employed, to lay or to relay 25,000 to 27,000 miles of track a year. We now have 93,000 miles of railway, of which about 60,000 miles are ten years old and over. Allowing for the postponement of renewals in past years, double tracks, etc., I estimate that these railways will require some 800,000 to 900,000 tons of rails per annum for the next four or five years, to relay their tracks. We are also building some 7000 miles of new railway a year, a rate of progress, however, which I believe we cannot maintain without great risk of running into unprofitable investments, and bringing about a fresh collapse; but even if we do build 7000 miles a year, the aggregate demands for rails in the United States would not exceed 1,500,000 or 1,600,000 tons a year, or a little less than the estimated capacity of the steel works alone for 1882.

We are not likely, therefore, to import many rails from Europe, except occasionally on an emergency, and as a reminder to our manufacturers that there are other railmakers in the world; but if we do, let us not ask the foreign mills to grind off the ends of the rails, to make them exactly of even length, a foreign practice which Mr. Sandberg so justly warns us against. Neither shall we ask them to notch steel rails, except in rare instances, as most of our roads have now adopted, or are adopting, angle fish-plates, to which the notching is transferred, thus preventing creeping through the shearing resistance of the bolts; but we shall undoubtedly require them to drill all holes for the latter, and we find that a round hole, one inch in diameter, with a $\frac{7}{8}$ inch bolt, allows sufficient play to provide for contraction and expansion.

I made some experiments upon rail joints some years ago, which indicated rather better results than those given in Mr. Sandberg's Table No. I, Appendix II. I found that the Erie standard steel rail of 63 pounds weight per yard, upon solid bearings two feet apart in the clear, required the application of a weight of 60 tons on the head in the centre between the bearings to break it; that the old joint, composed of two flat plates, broke with 20 tons similarly applied; that the composite joint, consisting of one flat plate and one angle plate, broke with a weight of 25 tons, while the Erie standard joint, of two angle-plates, 24 inches long, required 34 tons to break it. The flat-plate fishing was, therefore, 33 per cent., the composite joint 41 per cent., and the standard angle-joint 57 per cent., as strong as the

solid rail, and the angle-plate fish showed such marked superiority that our adoption of it was fully confirmed.

But we are especially obliged to Mr. Sandberg for the details and blank forms which he gives us of his methods of inspection. I wish particularly to call your attention to the blank for the inspection book, Appendix IV, and to the form for reports, Appendix V. I think it would be well for us to adopt them for the use of our own inspectors in this country.

I do not, however, quite understand that clause in his specification for *steel* rails (page 30), which, under the head of "Tests," provides that: "2d, The rails must carry, in the same position, a load of -- tons without breaking; after this the flange of the rail will be cut, and the rail broken. The fracture must show perfect welding, especially in the head." I thought it was a peculiarity of steel rails, that they were made from a single piece or ingot, and I am puzzled to imagine how the foreign makers contrive to get welds into them.

You will notice that nearly all my remarks refer to steel rails. I ought to have said so before, but the fact is, that when we now talk or think of rails, it is almost always of steel rails, for the days of iron rails are numbered. Already we see, when we examine one of these diagrams of prices of iron and steel rails, which look so much like the profile of a railway preliminary survey through a mountainous country, that the iron rails average only $5 or at most $10 a ton cheaper than steel rails; and the time cannot be far distant when steel rails will be produced as cheaply as iron. Indeed, I do not believe that any of our roads are now so poor as to be able to afford to buy iron rails, except, as I said before, upon an emergency.

The thanks and support of all railway men are therefore due to Dr. Dudley for his resolute attempt to ascertain the best composition and characteristics for steel rails. He may not as yet have gathered all the necessary data; his present conclusions may have to be corrected with reference to further facts; but I know that he is rendering valuable service, and I believe that he is on the right track.

I quite agree with the remark made by Mr. William Sellers, that the consumer should not undertake to prescribe to the manufacturer how he is to make his rails, nor what materials he is to employ, but should leave him free to select the surest and cheapest way of making a good article. The consumer is interested in the results only; but as

the desired result in this case is that the rail shall wear as long as possible, and as steel rails wear out so slowly that we cannot know for many years which make of them is going to give the very best satisfaction; and while the economical results are so important, I believe that it is our duty to endeavor to ascertain the characteristics of the best rails, to assist the manufacturer to repeat his successes, and to avoid his failures, by giving him whatever data we can gather as to the rails in our tracks.

This, as I understand, is what Dr. Dudley has undertaken to do, by ascertaining the chemical composition and physical characteristics of the rails which have best or worst worn on the Pennsylvania Railroad. While I will presently mention some considerations why his experiments may need to be revised, I yet recognize that he has done and is doing a great public service.

I must say, however, that the chemical composition which he recommends does not strike me as a particularly soft steel. From the criticism which he has received here, I doubt whether Dr. Dudley himself now thinks that he has as soft a thing as he at first imagined. He advises that the phosphorus should not exceed 0·10 of one per cent., the silicon not above 0·04, and that the carbon should aim at 0·30, and the manganese at 0·35 of 1 per cent.

Now the Erie specification of 1876, adopted after consultation with Mr. Holley, reads: "Not less than $\frac{30}{100}$ths nor more than $\frac{45}{100}$ths of 1 per cent. of carbon; not more than $\frac{10}{100}$ths of 1 per cent. of phosphorus, and not more than $\frac{16}{100}$ths of 1 per cent. of phosphorus and silicon taken together. It may contain manganese, but shall be substantially free from other impurities."

In view of the fact that this was the state of the art about the time Dr. Dudley began his labors, and that really *soft* steel, that which we use for our boiler plates, only contains 0·08 to 0·15 of 1 per cent. of carbon, the term of "soft steel," which is much dwelt upon by the author, is rather a misnomer. What he is engaged upon is the ascertaining what are the exact chemical compositions which give absolutely the best rails, and how these shall be distinguished by physical tests. To accomplish this satisfactorily, he will have to gather a good many more data.

I think exception may be taken to the method by which Dr. Dudley has undertaken to ascertain the loss of weight sustained by each rail. Having taken up the whole rail, presumably about 30 feet long,

he has cut out a slice from it, somewhere, one-half an inch thick, and from this slice he has, by weighing and measuring, ascertained the loss of weight. Any one who will caliper a worn rail throughout its whole length, and thus ascertain how much greater is the wear in some spots than in others (differing $\frac{1}{16}$th of an inch in sections 2 inches apart in many cases), will have serious doubts whether Dr. Dudley has in each case hit upon the particular half inch which is a fair representation of that wear.

It seems also difficult to accept the inference to which this method of procedure leads him, when he says that "rails rolled at the same mill, at the same time, and with the same thickness of web, and same shape of foot, differed from each other in the original weight (*as computed*) from $1\frac{1}{2}$ to 3 pounds per yard." Such is not our experience with Erie rails. We find that when the rolls are freshly turned up, the rails run about $62\frac{3}{4}$ pounds per yard, and that this weight is gradually increased, as the rolls wear, to about $63\frac{1}{4}$ pounds per yard; each invoice of say 1000 tons (the shipments are generally of about this amount) averaging as near as may be the 63 pounds per yard represented by the standard template. Here, therefore, we have a variation of only $\frac{1}{2}$ pound per yard, which is the limit assigned by our specification, and, as I said before, it seems hard to believe that on the Pennsylvania Railroad it could have been so much as $1\frac{1}{2}$ to 3 pounds per yard.

I scarcely need to point out that if errors have thus crept into Dr. Dudley's estimates of the loss of weight sustained by each rail, his reasoning and conclusions will be affected throughout. I recognize the difficulty of getting at the wear of a rail the exact original weight of which is not known, but I believe Dr. Dudley will yet find better methods than that of computing it from a half-inch slice. Perhaps more satisfactory results may be reached by a careful calipering of the stem, head and foot of the rail, and ascertaining its density, from which to deduce its original weight, deducting therefrom, to ascertain the wear, the actual weight of the worn rail. In fact, as he finally resorted to the method of averages, Dr. Dudley would have reached nearly the same result by assuming that the rails originally averaged of standard weight, and weighing together each group of eight rails, upon which he bases his deductions of wear.

I may also say a word as to the comparisons of wear upon the

upper and lower sides of curves. These would have been more satisfactory if we had been told the differences, if any, which exist between the elevation of the outer rail on these various curves; also the speeds at which trains generally run over them. The elevation of the outer rail being intended to overcome the centrifugal force, and this, of course, varying with the speed of the train, it is quite practicable for the track foreman to throw the wear upon the inner or the outer rail, by changing the elevation, or, in a less degree, for the locomotive engineer to do the same thing, by running faster or slower than the speed for which the curve is elevated. I hope, however, that Dr. Dudley will check over and continue his investigation. It is not improbable that the result will be still further to confirm his theory.

The railroad men of the whole country, who are specially interested in reaching sound conclusions on this subject, can materially assist in gathering additional data, by taking care to preserve rails which have worn exceptionally well or ill in their tracks, and sending them, with a statement of the particulars of each case, either to Dr. Dudley, if he will consent to test them, or to some of the Bessemer works from which they obtain their steel. All of these have competent chemists; they are vitally interested in maintaining a reputation for making good steel rails, and they would doubtless be glad to make arrangements to analyze and test any specimen rail which might be sent to them, in order to ascertain the causes of its excellence or deficiencies.

In listening to Dr. Holley's paper upon Rail Sections, I was reminded of De Quincey's ideal murderer, who, beginning with a murder, which he thought little of at the time, had gradually fallen to robbing, drinking and Sabbath-breaking, and so on, down to incivility and procrastination. For, having adopted some years ago a rail pattern which I have never recommended to other roads, nor claimed credit for, I now unexpectedly find from Dr. Holley's paper that 62 per cent. of modern rail sections are fashioned after that pattern, that the considerations which guided me are thought worth enumerating, and that it furnishes a good text from which to preach a sermon to railroad men. I hope, however, to satisfy you that I am not entitled to as much notice as Dr. Holley has been pleased to give me.

As Mr. Welch has told you, we were both in 1874 members of a

committee of the American Society of Civil Engineers to investigate the best form of rail sections. He then called my attention to the success of the thin flange and stem of his pattern of 1866, and I adopted them for the Erie Railway, which was then much in need of a standard steel rail section. The beveled head was furnished, ready made, by the sections of old rails which I examined, and was confirmed by the templates of worn wheels, twenty or thirty in number, which I obtained from locomotives and cars. I simply gathered the data, and was guided by them, as any one would have been in my place, and as in fact others had been, for I am informed that Mr. Sayre, of the Lehigh Valley Railroad, and Mr. Fritz, of the Bethlehem Steel Works, had designed and rolled a similar rail as early as 1870.

The Erie rail was originally designed to weigh sixty pounds per yard, this being the limit at that time imposed by the managers of the road. It took some months to get the pattern accepted, some of the rolling-mill managers declaring that it could not be rolled without producing a large percentage of imperfect rails. Mr. L. S. Bent, however, the superintendent of the Pennsylvania Steel Company's Works, thought differently and determined to try it. He found that the percentage of imperfect rails was actually less than with other patterns then in use, and he designed and introduced a number of steel rail sections on the same principle, which have become standards.

Some three or four thousand tons were rolled and laid of the original Erie sixty-pound pattern, and they have stood very well, but Dr. Holley having suggested that the thinness of the foot, in proportion to the head, might cause dangerous internal strains in cooling, and thus make the rail brittle and dangerous, a thickness of one-sixteenth of an inch was added to the foot, increasing the weight to sixty-three pounds per yard, and this has been the Erie standard section ever since. As I said before, in my opinion the Pennsylvania Railroad section of sixty-seven pounds per yard is better, as likely to wear about 50 per cent. longer.

Up to a certain point, there is an advantage in diversity of railroad practice. So long as the best device for a particular purpose is not ascertained, there is a necessity for experimenting, and the resulting variety of design. When, however, the best pattern is approximately agreed upon, the effort should be towards uniformity. This point seems now to have been reached about steel rail sections, although I

had no idea this was the fact, and I hope the railroads will take advantage of the economy which Dr. Holley has shown us to result from the adoption of uniform standards.

He has called our attention to the importance of uniformity in fishing, and especially in spacing the bolt-holes, but he has not told us which he considers the best practice. I venture to present a drawing of the Erie standard joint. (See accompanying plate.) There is nothing novel about it, but the points which we think meritorious are the following:

1st. The holes in the rails are placed as far from the end as we deemed practicable. The centre of the first hole is pitched an even 4 inches from the end of the rail, and the second hole is 6 inches beyond this, or 10 inches from the end.

2d. These holes are drilled in all cases, are 1 inch in diameter, and as near the neutral axis of the rail as we could get.

3d. The fishing is done with angle plates, which we find about 70 per cent. stronger than flat plates. The notching, which is in the fish-plate and not in the rail, is spaced $3\frac{1}{2}$ inches from one end, and 5 inches from the other, so that when the plate, which is reversible, is applied to both sides of the rail, the notches are staggered sufficiently to avoid splitting the ties with the spikes.

4th. The allowance for expansion is made in the fish-plate, the two centre holes being spaced $8\frac{9}{32}$ inches apart. As the next holes are, of course, 6 inches beyond the centre holes, and the plate is designed to be 24 inches long, it will be noticed that if the man at the shears cuts it off at the right length, and the man at the punch centres it exactly, the distance from the centre of the end holes to the end of the plate will be precisely $1\frac{55}{64}$ inches. I hope that Dr. Holley, who says he was appalled at the thought that the mind of man can hit perfection in spacing fish-plate holes within the 64th of an inch will see from this brief exposition of the process that it is more easy to accomplish than he supposed.

5th. The holes in the fish-plate are made oval to allow for expansion and contraction. The bolt, which is $\frac{3}{4}$ of an inch in diameter, is upset under the head to fill this oval hole, and thus prevent turning. It is provided with a hexagonal nut, under which we generally place a thin washer of wrought iron. We have very little trouble from nuts getting loose, so little indeed that, while we have experimented

with a number of lock-nuts, we have not deemed it necessary to adopt any of them.

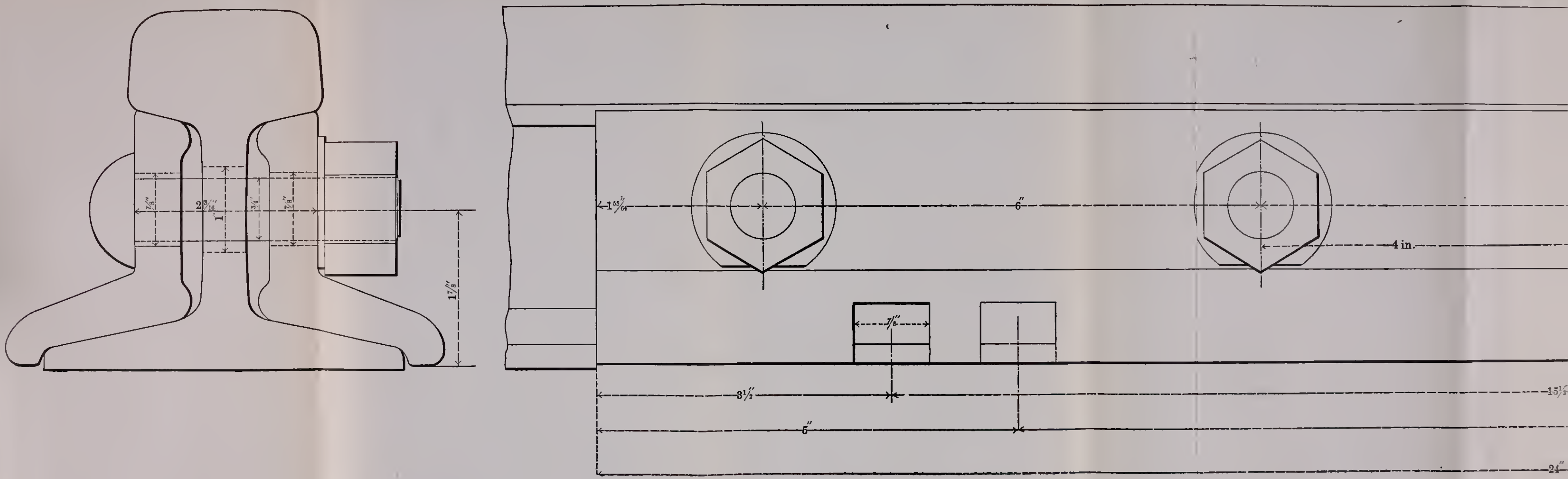
But I fear I am becoming wearisome by my discussion of these details, which would be more appropriate before a special committee on this subject, such as that appointed in 1874 by the Society of Civil Engineers. The main point before you is that so well made by Dr. Holley, of the importance and economy of uniformity in rail sections and fastenings. Of that we have had some experience. We had on the Erie railway, when the new steel section was adopted, 12 patterns of steel rails, 29 patterns of iron rails, and 96 different styles of fastenings. These caused no end of annoyance, delays and expense, in matching or mismatching them, taking up and changing about long strings of rails, and in the large stocks which it was necessary to keep for repairs. This has all been done away with by the adoption of a single pattern of rail and of fastening, and the resulting economy fully confirms all that Dr. Holley has said. He has shown us that the railroads of this country can save several millions a year by adopting uniform rail sections, and as, unfortunately for him, he cannot patent his idea, it only remains for the railroads to adopt it, and to thank him for his paper.

Dr. C. B. Dudley, Altoona, Pa.: In rising to close this interesting discussion I want, in the first place, to thank every one who has contributed to it for his full and open criticism. The work which has been done on steel rails, and which has been discussed here during these two days, was not done to establish any pet theories, nor to make out that any person was great or any person small, but with a sincere desire to get at what is the truth in regard to the wearing capacity of steel rails. There are enormous commercial considerations involved in this question, and, as I look at the matter, the more honest criticism and fair discussion there is, the more likely it is that the truth will appear.

And first I would like to say that it seems to me very little has been said here upon the main conclusion of the paper, namely, that the softer rails give the better wear. All sorts of side issues have been discussed; but this point, which is really the principal one at issue, has been largely ignored, and I cannot but feel that it still remains unshaken.

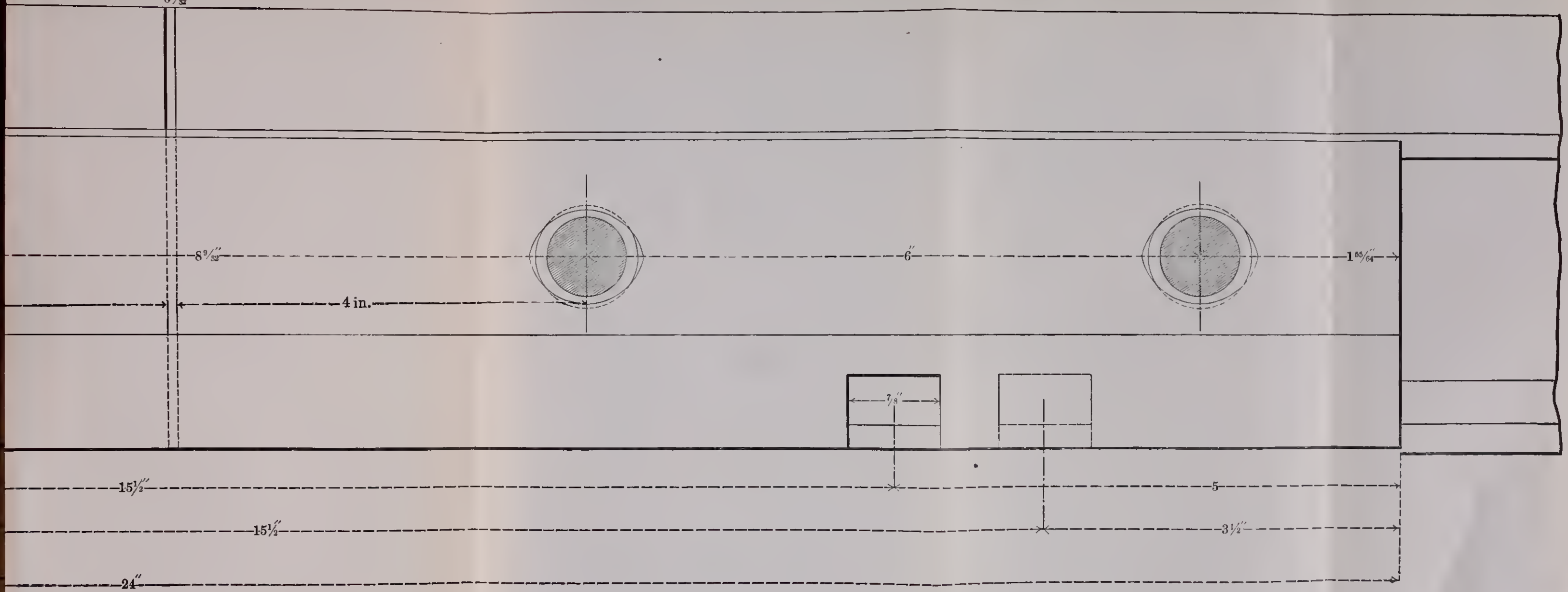
With regard to chemists and chemical work, there has been consid-

STANDARD RAIL JOINT, — N.Y.L.E.& W.R.R.



.Y.L.E.& W.R.R. FULL SIZE

Chanute.



erable said tending to throw discredit on chemists and their work; and while I believe that there have been in the past, are now, and may be in the future, a good many poor chemical analyses made, I also believe that chemists are, as a rule, as honest and competent as gentlemen who belong to other professions. There are chemists who are chemists, and chemists who are not chemists.

The determination of manganese has been called in question. Now I think the chemist at almost every steel works in the country will tell you that, in his experience, the manganese differs in different parts of the same ingot. Mr. T. T. Morrell, chemist of the Cambria Iron Company, whom I believe to be a thoroughly competent and honest chemist, tells me that he has often found different amounts of manganese in different parts of the same ingot. Come with me to Altoona, and I will take you into the machine shop where steel is being cut and shaped, and I will show you that it is often necessary to stop the lathe or planer and take a cold chisel to cut out a hard spot, or else run the risk of breaking the tool. This hard spot is simply a part of the spiegel which is not thoroughly mixed with the mass when the steel is made. In the rapid methods by which steel is at present manufactured time enough is not allowed for the spiegel to become uniformly mixed. What wonder, then, that chemists find different amounts of manganese in what is supposed to be, but is not, the same steel. Indeed, I believe it is possible for the borings from one bore hole in the same ingot to be given to two chemists and to have them find different amounts of manganese, and yet both analyses be correct. And so I say to the steel makers, "Make uniform steel, and we, as chemists, will tell you what there is in it."

With regard to the determinations of manganese in the series of rails we are discussing I would say, I wish Mr. Wells was here, that you might see him for yourselves. When I began this work, I wrote to my old instructor in chemistry, Professor O. D. Allen, of the Sheffield Scientific School, to recommend me some one to help me. He replied that if Mr. Wells would come he could heartily recommend him. He had had two years' experience since his graduation, and, said Professor Allen, "I regard him as the best analytical chemist that has graduated under me." And I may add that both Professor Drown and myself graduated under Professor Allen.

Still further, it is simply impossible that any errors, either in the chemical analyses or the physical tests, should have had any influence

in establishing the point that the softer steel gives the better wear. This follows from the way in which the work was done. First the physical tests were made, then the analyses, then the tonnage was computed, and finally the loss of metal was determined. So that we knew nothing about a rail until all the chemical analyses and physical tests were made. Furthermore, some of the rails that were selected as faster-wearing rails, when we came to get the rate of wear, were found to be slower-wearing rails. So that no previous bias of mind, or, as it seems to me, no possible errors in the work could influence the result.

Again, with regard to sulphur and copper, it is said that these are of vital importance, and should have been determined. In answer to this I would say: Where is the man that can affirm, and back his statement by any analysis, that sulphur and copper have any influence on the wearing capacity of steel? I do not say that these elements do not have an influence on wear, but when this investigation was started the best information that I could get was that sulphur and copper were of vastly more importance to the steel manufacturers than they were to the consumer. And so I say that I believe the sulphur and copper are of importance to the makers of steel, but of not so much importance to one studying its wearing capacity. If you want to know the sulphur and copper in these rails you may determine them.

Probably no one has thought over the question why some of the rails in this series seem to be exceptions to the general law more than I have. This suggestion in regard to sulphur and copper, and other undetermined substances, and especially, in my judgment, oxide of iron, furnishes a possible solution of the problem. If we knew every foreign substance which these rails contain I doubt not but that some of the anomalies would be explained. And I would here like to ask chemists who have time to devote to such studies, to give us a method for determining oxide of iron in steel.

Another point made was the influence of heavier locomotives and cars on the wear of rails. If I understand this criticism it is this: Your slower-wearing rails had lighter-wheel tonnage for at least a portion of their life—the earlier portion—while your faster-wearing rails have had almost altogether heavier-wheel tonnage. In reply, I say the slower-wearing rails had during the latter part of their life the same heavier-wheel tonnage that the faster-wearing had. All the rails were taken out of the track at the same time, and, consequently,

so far as I can see, the comparison of the wearing capacity of steel with its quality is strictly a fair one.

Again, in the course of this discussion—not a few times—the exceptional cases, the cases where individual rails did not conform to the general law, have been taken out and held up prominently before us, as though these individual and exceptional cases were the only thing we should consider. Now, I submit to you that this is simply trying to overthrow a law by the exceptions to it, or, in other words, to nullify the teachings of a large number of samples by the teachings of a few exceptional cases, and I submit still further that this method of proceeding is neither good logic, nor fair, sound deduction.

I must not omit to comment on the remarks of those speakers who have refuted conclusions which I did not advance, and have then considered my position, namely, that the softer rails give the better wear, as completely demolished. A notable case of this kind is Mr. Kent, who, because he does not find that there is a direct relation between the loss of metal and the carbon, phosphorus, silicon or manganese, or phosphorus units in this series of rails, affirms that I have not solved the whole problem of wear, and, *ergo*, the softer rails do not give the better wear. I beg to remind him that I have never said that I had solved the problem of wear. I expressly say I have not solved it, but I do not see how that affects the main question; nor do I see, because there is no direct relation between carbon and loss of metal, that it is impossible for me to take a series of rails which have been in service and find by a study of them what chemical composition and what physical properties are, in general, characteristic of those rails which have given the best service. This I claim to have done, and the conclusion seems to me so plain that he who runs may read, namely, that the softer rails give the better wear.

With regard to Mr. Metcalf and his attributing all the troubles of steel to nitrogen, I think it may fairly be said, first, that Mr. Metcalf brings no proof to show that nitrogen is the bane of steel; and second, if it is, the natural conclusion would be that no steel could be made except by the crucible process, which would undoubtedly be a satisfactory conclusion for crucible steelmakers, like Mr. Metcalf, but would hardly satisfy the stockholders of the Bessemer works, or stop their making steel with nitrogen in it in the future. With regard to another criticism of Mr. Metcalf's, that the question of flow had not been considered, I would say that I think there is very little evidence of

flow in this series of rails anyway. And so I asked Mr. Metcalf how the flow influenced the loss of metal by wear. He replied that flow squeezes the metal toward the flange and then the flange rubs it off. To this I made reply, that the flow, whatever there is of it, must be away from the forces which produce it. Now, both the pressure of the flange against the rail and the coning of the wheels would cause the metal to flow away from the flanges instead of toward them, and consequently I do not see how you are going to get metal there for the flange to rub off. The flow must be in the other direction, or away from the flanges. Although a few of the rails in this series give evidence of having a little metal pushed off out of place by reason of flow, yet the metal is there. It is not worn off, and the question we are studying is loss of metal by wear.

One or two points further, and I am done. It has been said, "You have not exhausted the question yet. More study must be put upon it." No one is more conscious of the truth of these statements than I. I do not pretend to have exhausted the question. I wish there were fifty workers in this field. But I believe that the results that we are discussing are the best information that we now have upon the question as to the relation between the wearing capacity of steel and its chemistry and physics. I would not at all affirm that this will be the best information on the subject five years from now. But I think that man does his life-work best who lives up to all the light that he has in his own time. And so I ask you to utilize this work, to act upon it and guide your practice by it until something better is obtained.

Finally, I have been accused of trying to teach the steelmakers how to make steel, and it is to be supposed that they know already much more about that point than I do. Now, if any one thinks that such has been my aim, or has ever been in my thoughts, he has certainly misunderstood me. What I am striving for is to tell the steelmakers what we want, not how to supply this want. This whole question of the fitness of material for the purpose for which it is intended is in its infancy. We are doing something toward studying it at Altoona. The principle which governs us there is that the kind of service that is to be required of the metal must determine what kind of metal shall be used. Because softer steel gives better rails, we do not think softer steel will give better crank-pins. In crank-pins we require stiffness, which comes with harder

steel. But in rails, in tires, in bridge rods and in boiler plate we are, so far as our knowledge now goes, inclined toward soft steel. And all the information which we have thus far been able to accumulate in regard to these kinds of service confirms our position and justifies our conclusion.

And now, how can the best results be obtained in trying to decide upon the quality of material best fitted for any kind of service? I do not see that the steelmakers can study this question alone, for after the steel leaves their hands they know very little of its behavior. It does not come under their personal observation and study. It seems to me, therefore, that the question can only be studied by both the consumer and the producer working together. I cannot but regard that the interests of the consumer and the producer in this matter are one, that neither can solve the question alone, and so I ask you to work with, rather than oppose me, to utilize the information that is gained, so far as it is gained, and to constantly hold in mind the necessary dependence of both producer and consumer upon each other.

Velocity of Light and Electricity. The probable identity of wave velocity in light and electricity has been established in various ways. Weber and Kohlrausch measured the quantity; Sir William Thompson and J. Clerk Maxwell experimented upon the electromotive force; Ayrton and Perry operated upon the electrostatic capacity. The extreme range of velocity in the various results was about 10 per cent.; the mean of all the results appear to correspond precisely with the velocity of light. The meaning of this accordance may be explained as follows: Suppose two parallel plain surfaces, each charged with a unit of positive electricity and placed at unit of distance; they will repel each other with a unit of force. Now suppose that they are both set in motion in the same direction, but remaining parallel and at the same distance; they will produce the effects of two parallel currents of the same kind and will exert a mutual attraction which will increase with the velocity of motion. It is possible to conceive of a velocity such that the attraction resulting from the motion will exactly counterbalance the repulsion which arises from the similitude of electricity; this velocity is precisely that of light.—*La Lumiere Electrique.* C.

BURNISHING AND DUCTILIZING STEEL.

By Jacob Reese, Pittsburgh, Pa.

Read at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17, 1881.

I have discovered a new method by which steel and other metals may be burnished by the automatic action of the burnishing machine, and by which the cost is greatly diminished and more perfect work produced. And in addition to the polishing and burnishing action of

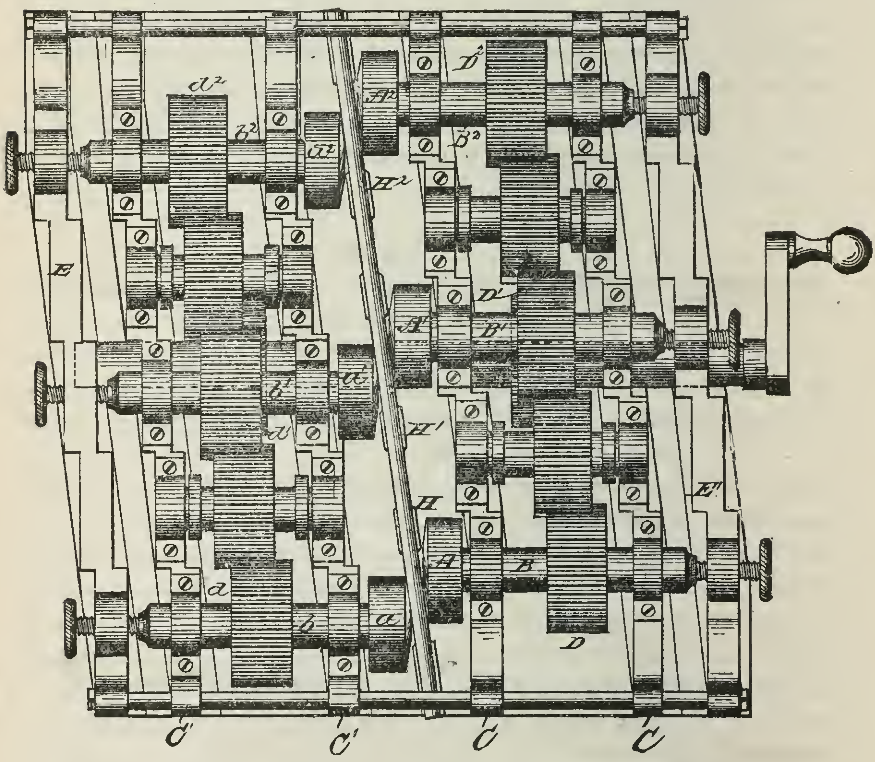


Fig. 1.

the new process, I have discovered that by a certain practice in the process of burnishing, the metal under treatment may be permanently increased in diameter of its cross section, and its ductibility increased

from 30 to 90 per cent. at a temperature ranging from 60° to 250° Fahr.

The machine is shown in the accompanying drawings; Fig. 1 is a

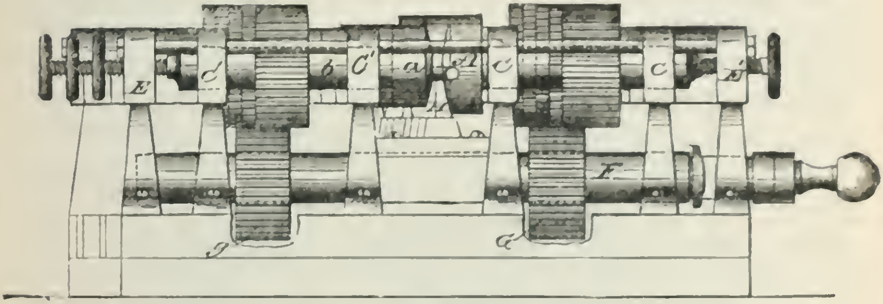


Fig. 2.

top view of a continuous-disk machine, Fig. 2 is an end elevation of the same, Fig. 3 is a top view of a set of conical disks and Fig. 4 is a

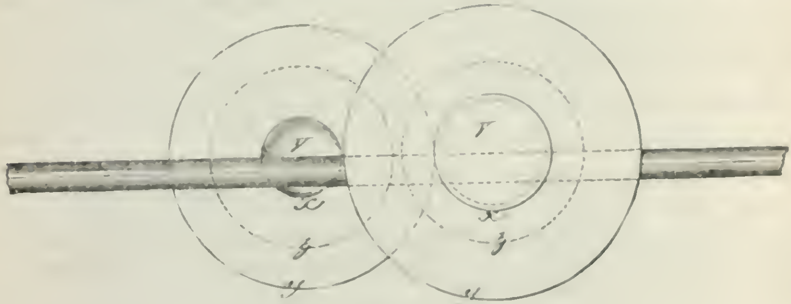


Fig. 3

diagram showing the working face of one disk and the back of another. Like letters refer to like parts wherever they occur.

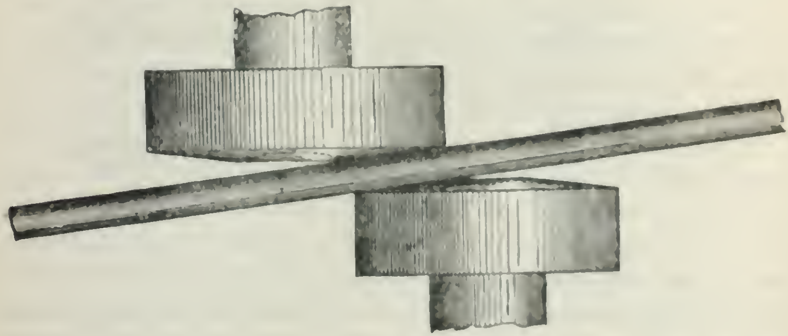


Fig. 4

The series of disks, which in the present instance are six in number, are arranged so that they all operate upon the bar at one and the same time. The faces of the disks are slightly conical and the centres of the faces are turned concave, so that the working-face of each disk extends from the edge of the periphery to the outer edge or line of the concave portion of the disk. The disks are of two sizes, the larger being about sixteen inches in diameter and the smaller about fourteen inches. The large disks A A' A^2 are placed upon the same side of the working-line and the small disks a a' a^2 are placed on the opposite side, the object being to secure a downward bite upon the bar by the large disks and an upward by the small disks, thereby keeping the bar down firmly on to the rests. These disks are mounted on suitable shafts B B' B^2 and b b' b^2, which are set in the standards or housings C and C' in such a manner that the large and small disks are not directly opposite to each other, but bear such relative positions as will bring the outer edge of the working-face of each disk directly opposite to the inner line of the working-face of the adjacent disk. This arrangement is not absolutely necessary, but renders the construction simpler than other modes of arrangement.

The disk-shafts are provided with pinions D D' D^2 and d d' d^2, which mesh into idlers or pinions mounted on shafts, which are set into the standards between each pair of disks. It is necessary to have idlers for the disk pinions to mesh into, because if they were to mesh into and communicate motion directly to each other every other disk on the same side of the working-line would revolve in an opposite direction, and consequently prevent the mechanism from working.

E E' are end housings, provided with suitable adjusting-screws for the purpose of setting the machine for any given size of work, to adjust the disk-faces in a parallel line and regulate the pressure upon the metal operated upon. F indicates the main driving-shaft, which is provided with pinions G, g, which mesh into the central disk-pinions D, d. H H' H^2 indicate rests, which are set in line beneath and between the disk-faces. These guides or rests are slightly less in width than the diameter of the piece of metal to be operated upon, and for ordinary work they are adjusted to keep the centre of the bar a little below the centre of the disks, that being necessary in order that the resultant action of the forces exerted by the movement of

the disks may cause the bar to feed forward as it rotates. The forward speed will depend upon the altitude of the rests in relation to the disk-centres, and if they are adjusted so that the centre of the bar is on the same line as the centre of the disks it will rotate, but without forward or backward motion. If the rests are adjusted to throw the centre of the bar above the centre of the disks, it will have a backward movement as it rotates.

When power is applied to the main shaft *F*, to rotate the same, the pinions *G g* communicate motion to the central disk pinions *D d*, which turn the idlers and thus communicate motion to the other disk-pinions, causing all the disks to rotate uniformly. In order to fit these disks for burnishing and ductilizing iron and steel it is necessary that their working-faces should be trued and highly polished, and this I accomplish in the following manner:

The machine having been adjusted for any given size of work and the guides or rests being in a line and adjusted to the proper height, I take a square piece of hard wood of suitable thickness and place it upon a rest in front of the machine. I then oil the disk-faces and sprinkle them with emery and finally enter the block between the disk-faces, when it will be caught and drawn slowly forward, thus truing and polishing the entire train of working-faces at one and the same time, and also polishing and truing the working-faces of the rests. The machinery is now capable of burnishing and ductilizing the metal when properly adjusted for that purpose, and this adjustment is a matter which will require considerable skill and care upon the part of the operator, as a degree of pressure is necessary in some cases which would be entirely inadmissible in others.

By referring to the drawings it will be readily understood that when the rests are adjusted to any given height the feeding of the bar will be uniform and constant, and therefore the only method of increasing and decreasing the frictional action upon the surface of the metal will be by regulating the pressure as occasion may require. The greater the friction the greater will be the tractive force which tends to draw or film the surface of the metal. The ability of the metal to resist this drawing force depends upon the attraction or cohesion of its particles. This varies in different metals and in the same metals at different temperatures, being greatest at the lowest and least at the highest temperature; and in iron and steel it depends greatly upon the amount of carbon in combination with the metal. Now, it is evi-

dent that when the bar is put into the machine its temperature will be gradually raised by the frictional action at each successive pass, and therefore become less and less able to resist the tractive force, and that as its temperature increases the metal will gradually expand in diameter, so that if all the faces are previously adjusted to exactly the same distance apart, not only will the metal become less able to resist the tractive or drawing force, but the increased temperature of the metals will, by causing such expansion, develop more pressure and frictional tractive force, so that the metal will then be very liable to draw or scab. The disk-faces should therefore be so adjusted as to bring the greatest pressure at the first pass and to apply a little less at each successive pass until the burnished bar is completed.

In conducting the operation the object is to secure sufficient pressure to compress the inequalities and to develop enough frictional or tractive force to overcome the attraction of cohesion of the particles composing the scale on the surface of the metal, yet not enough to overcome the force of cohesion of the particles which will then form the surface of the metal itself. If the metal has a great force of cohesion and does not possess a tough, tenacious scale, this may be readily effected ; but where the conditions are opposite great care must be had and a constant watch kept for signs of filming. Therefore, as it is imperatively necessary that certain degrees only of pressure, frictional action and tractive force be applied or developed upon the surface of the metal, it is necessary, first, that the metal should have been previously rolled to an exact or uniform gauge, so that when burnishing an undue amount of pressure may not be developed upon its surface at any point; secondly, as the ability of the metal to resist the tractive force is less when at high temperatures, it should be operated upon when in a cold state, or at a temperature not exceeding 500° F.

The machine being in condition, having its working-faces trued and polished, and the metal having been properly prepared by rolling to an exact gauge, a test-bar may be entered and the working-faces gradually tightened up after each pass until a point is reached at which the films begin to show upon its surface. This is an indication that the pressure is too great, and the tightening-screws should be relieved a little and the test-bar again entered, a careful watch being kept for further filming. If none appears the machine is properly adjusted for that sized bar; but if films still continue to form, the pressure

must be further decreased until a point is reached at which no films are formed. It is not necessary, however, that the machine should always be adjusted in the manner just described, as burnished bars may be sometimes produced, although a very light pressure is used, as, for instance, where the scale upon the metal is not tenacious and the surface of the metal is very smooth; but in all cases the pressure will be light in comparison with that which is required for rolling and extremely light when compared with that required for cold-rolling. I am unable to give the required degree which will be necessary in all cases, as I find that the pressure varies upon any given point, according to the difference in the diameters of the bars operated upon, the larger diameters burnishing under heavier pressure than the smaller; and, moreover, the pressure upon any given point may vary accordingly as the width of the burnishing-faces used varies. The wider the disk-faces the greater will be the amount of frictional action and tractive force upon any given part of the bar in any given time, and consequently the less must be the pressure. The converse of the proposition is also true, viz., the narrower the disk-faces the less the frictional action and the greater the pressure admissible; but the working-faces must never be made very narrow, as in such case so great a pressure would be required to develop the frictional action which is necessary that the operation would be entirely changed and cause a reduction of the metal and displacement of its particles, as in rolling. Finally, I find that the pressure may vary with the different temperatures and natures of the metal operated upon. Therefore no precise rule can be adhered to for all cases, except, first, to have all stock previously rolled to a uniform gauge; secondly, to have all the burnishing-faces turned perfectly true with a high polish; thirdly, to have the burnishing-faces constructed of sufficient width to develop a sufficient amount of frictional action when a light pressure is applied. Then feed the bar at a proper temperature and apply the pressure from an exceedingly light one to the highest the metal will stand without filming.

The machine being in the condition specified and adjusted as specified, rough bars of metal in a cold state are inserted, one at a time, between the receiving-disks. They are caught, rotated rapidly and drawn forward, traveling forward with a speed of from one to sixty feet per minute, according to the size of the bar, height of the rests and speed at which the disks rotate, and are delivered per-

fectly straight, of a true cylindrical form and highly burnished. In some cases, however, when the bars are of a large diameter and are covered with a tough, tenacious scale, additional passes may be required to accomplish this result.

When burnishing cold metallic bars, I prefer to place narrow pans, containing petroleum or other oil, beneath the second and third pair of disks. These pans are of sufficient size to contain the required quantity of oil and are shaped so as to form a sheath or trough, in which the lower portions of the disks revolve. The effect of the application of the oil is to prevent the working-faces of the disks from scratching and marring, and it also has a certain effect upon the finished product, of which I shall speak hereafter.

When it is designed to straighten and burnish metal directly as it comes from the rolls during its manufacture, the process should be the same as before specified, except that the metal should be allowed to cool to a dark red heat and a considerable quantity of water should be let upon the first set of disks and upon the bar, to reduce the temperature of the metal to the proper degree, so that in its rapid contraction the scale will be loosened. A steam or air blast should be used to carry the scale away, and the first and second set of disks will then readily remove any portion of scale which may still adhere to the bar. The metal will then pass from the first to the second set of disks, thoroughly cleaned and partially burnished and the second and third pair of disks will, by the frictional action, which also burnishes, heat the surface of the bar. Consequently, the lighter components of the oil will be vaporized, leaving the bar coated with carbonaceous matter, which is apparently forced into the pores of the metal by the action of the disks, and the bar, when burnished, is found to have a much finer appearance than if it had been burnished without the oil, and it is also enabled to resist oxidation to a greater degree.

For the purpose of illustrating the frictional action which may be obtained by the employment of disk-rolls, I will again refer to the drawings, in which Fig. 3 represents a top view and Fig. 4 is a diagram of a set of disk-rolls, the latter showing the back of a sixteen-inch and the working-face of a fourteen-inch disk. *V* indicates the concave portion of the disk-faces, which is five inches in diameter in the fourteen- and seven inches in diameter in the sixteen-inch disk. *x* indicates the inner lines or edges of the working-

faces; z the neutral lines, or those portions of the disks at which their rate of speed when working is equal to the surface speed of the bar in rotating; and y the outer edges or lines of the working-faces of the disks at their peripheries. It is well understood that the different portions of the disk-faces travel at different rates of surface speed, according to their position with relation to the disk-centres.

When a cylindrical bar is fed into the disks it will rotate, and, all portions of the surface of the bar being at the same distance from the axis or centre, will travel at the same surface rate of speed, and as the working-faces of the disks travel, having differential rates of surface speed, as before stated, great frictional action is produced on the working-surface of the disks and the surface of the bar; or, in other words, this result follows because the surface speed of the bar and disk-faces is the same only on the theoretical or neutral lines indicated by the letter z, the disk-faces traveling at a gradually increasing speed from the neutral line to the peripheries and at a gradually decreasing speed from the neutral line to the inner lines of the working-faces of the disks. If, therefore, as in ordinary practice, the working-faces of the disks are four inches in width from the outer to the inner edges of the working-faces, the area of the small disk will be $113\frac{96}{1000}$ square inches, and that of the large disk $138\frac{220}{1000}$ square inches, thus making a total area of $251\frac{316}{1000}$ square inches of frictional surface which slips or rubs over the surface of the bar at each revolution of the disks.

In addition to the frictional action of the disk there is that of the rests. A bar one and one-eighth inches in diameter will rotate (the rests being at the proper height) eight times to each revolution of the disks and feed forward about two inches to each revolution of the disks, or one-quarter of an inch to each revolution around its own axis. The length of the rest's working-surface is usually about five inches, so that the bar will revolve twenty times over the polished surface of the rest during the time any given portion of it passes from one end to another of the rest, and the rest acts the same as if the bar were rotated in a lathe and a burnishing-tool pressed against it with the same degree of force during twenty revolutions around its axis. Finally, there is the frictional action which arises from the forward movement of the bar over the burnishing-disks and rests.

One of the effects produced by the frictional action to which I have referred, or by the action of the heat developed by the friction, is to gradually expand the metal during the burnishing operation, so that as the bar travels forward the pressure will become augmented, unless the tightening-screws are adjusted lighter for the latter passes; and I have found that, when they are properly adjusted, the metal will in some cases retain permanently its increased diameter, which I have found in all cases to indicate a slightly decreased tensile strength, elastic limit and a greatly increased ductility in the metal. When, however, the tightening-screws were adjusted so as to compel the bar to retain its original diameter, I have found the elastic limit and ductility are the same as in the original state, and that the tensile strength remains almost the same as in the original state previous to the operation.

The following record of tests made by tension shows clearly the changes which are effected in the physical nature of the metal by the process:

Marks and Condition of Pieces.	Original Diameter.	Diameter after Fracture.	Original Cross-Section.	Cross-Section after Fracture.	Elastic Limit.	Elastic Limit per Square Inch.	Tensile Strength.	Tensile Strength per Sq. Inch Original Section.	Elongation in Five Inches.	Elongation.
	In.	In.	Sq. In.	Sq. In.	Pounds.	Pounds.	Pounds.	Pounds.	In.	Per Cent.
459 Rough	1·013	·955	·806	·716	51,000	63,283	88,235	109,486	·547	10·94
460 Bright	1·014	·853	·807	·571	47,500	58,823	86,795	107,486	·953	19·06
461 Bright	1·016	·851	·811	·569	49,500	61,058	88,395	109,035	·797	15·94
4 2 Bright	1·012	·969	·804	·737	48,000	59,674	88,305	109,782	·500	10·00
463 Bright	1·015	·910	·809	·650	47,500	58,704	87,365	107,972	·656	13·12
464 Rough	1·012	·952	·804	·712	48,000	59,674	87,500	10 8,782	·500	10·00

Nos. 459, 460 and 461 were cut from the same bar of steel. No. 459 was cut from the rough end, which was in the same condition as it came from the rolling-mill. Nos. 460 and 461 were bright, and were cut from the end that had been burnished. No. 459 broke just above the top centre punch-mark. No. 460 broke between the points of measurement. No. 461 broke at the top centre punch-mark.

Nos. 462, 463 and 464 were cut from one bar of steel, each end of which had been burnished. No. 464 was cut from the middle of the bar, which was rough as it came from the rolling-mill. No. 462 was cut from one end, and No. 463 from the other end, both of which had been burnished. No. 462 broke one and one-half inches above the top centre punch-mark. No. 463 broke one-half inch below the top centre punch-mark. No. 464 broke one inch above the top centre punch-mark. No. 462 did not break at its smallest diameter, which was midway between the points of measurements.

All the changes or phenomena which present themselves in the finished product show that the operation to which it has been subjected embodies some principle which has never before shown its effects in the product of any rolling, hammering or compressing operation, nor in the product of any burnishing, brightening or polishing operation known heretofore to the art of metallurgy. For instance, that indicated by the permanent increase in the diameter of the metal, although at the time it is confined between parallel surfaces and under pressure, the operation being conducted so that the heat developed does not generally exceed 250° Fahrenheit, which would not be sufficient, no matter how long continued, to anneal or ductilize heavy cold bars of steel by any known process.

When steel is required for structural purposes, it must be able to resist concussion, sudden shocks, rapid vibration and deflection, and give due warning, before final rupture takes place, by elongating within certain degrees. Consequently, heretofore, the steel has been made low in carbon to secure the required ductility, and therefore possessed a low tensile strength. The steel is annealed after its manufacture, to bring it back to its normal condition, and destroy in a measure the effects produced in its physical structure by the ordinary rolling operation, since all rolling, hammering and drawing leave the physical structure of the metal in an abnormal condition, produc-

ing hardness, brittleness and liability to rupture from concussion, vibration or rapid deflection.

Annealing steel reduces its ability to resist tensile, compressive and torsional strain, as well as its elastic limit, and increases its elongation or ductility. It is a slow operation, lasting generally from five to twenty-four hours, and leaves the metal covered with scale. By my process the ductility of the metal may be greatly increased without the formation of a scale upon its surface, and in a very rapid manner. In comparing it with ordinary annealing operations the following facts become apparent:

First. As the metal is previously rolled to an exact size, and the disk-faces are adjusted to exert exactly the same degree of pressure and frictional action upon all parts of the bar, the ductility of the metal should be constant and uniform at all points, whereas in annealing the temperature of the furnace varies at different parts, consequently the metal cannot be uniformly annealed.

Second. As the bars are all of the same size previous to the burnishing operations, the same degree of ductilizing action should be had upon each, and consequently they should all be regularly ductilized, whereas in annealing, the process is not automatic, and the bars are charged and drawn by hand, so that they are exposed to heat for irregular periods of time.

Third. The ductilizing effect is produced by this process at a heat never exceeding 500° Fahrenheit, and this is too low to deprive the bar of carbon; or, if it does so to any extent, it does it uniformly, whereas in annealing the bar loses considerable carbon, and loses it unequally, so that not only is the tensile strength reduced considerably, but it varies at different points of the bar.

Fourth. As the ductilizing of the metal is constant and uniform, its internal strains should be regularly and uniformly relieved, whereas in annealing the temperature always varies in different parts of the bar; hence its internal strains should be irregularly relieved.

Fifth. As the temperature in this process is very low and uniform upon all parts of the bar it remains perfectly straight in cooling, whereas in annealing the high and uneven temperature of the metal causes it to warp and become distorted in cooling.

Sixth. By my process I am enabled to ductilize metal at the rate of

one foot per second to one foot per minute, whereas annealing operations require several hours or days for their completion.

This process will be found to be peculiarly adapted to the production of steel shafting, piston rods, and also for very light work, such as burnished steel for pivots for watches and clocks, etc., in which latter case it is evident that the mechanism employed must be of a reduced size suitable for the work to be accomplished.

Comparing the mechanical effect of this process with other well-known processes, the difference is very marked. Wrought iron possessing a tensile strength of 50,000 pounds per square inch, and an elastic limit of 30,000 pounds per square inch, and exhibiting an elongation of 25 per cent., will, when cold-rolled by the Lauth process, possess a tensile strength of 68,600 pounds, and an elastic limit of 59,600 pounds, but the ductility is reduced to an elongation of 6 per cent. When such cold-rolled iron is annealed it is found to possess a tensile strength of 48,700 pounds, an elastic limit of 32,000, and an elongation of 15 per cent.

Professor R. H. Thurston, in his paper on "Mechanical Treatment of Metals,"* said: "All known and actually practiced methods of so altering the character of the metals used by the engineer, involve, directly or indirectly, the elevation of the original elastic limit of the material." In this process, however, the elastic limit is slightly reduced.

In conclusion I would add, that the phenomena exhibited in this process invite further research into the laws of molecular physics.

Electric Light in the French Lighthouses.—Four of the most important French lighthouses have already been provided with very powerful electric apparatus and with machinery for furnishing sound signals during stormy or misty weather. It is proposed to apply similar apparatus to the forty-two others. The total cost is estimated at 7,000,000 francs ($1,400,000) for the electricity, and 1,000,000 francs ($200,000) for the fog horns. This expenditure is very light in view of the protection which it will afford to the immense capital represented by the 225,000 ships which annually visit the French harbors.—*Les Mondes.* C.

* *Metallurgical Review*, vol. i, page 1.

INDUSTRIAL EDUCATION FROM A BUSINESS STANDPOINT.

By John S. Clark.

An Address delivered before the Philadelphia Board of Trade and the Franklin Institute, June 6th, 1881. With additions by the author.

If I were to come before your Board of Trade with a statement that there had been discovered within easy access of Philadelphia a bed of iron ore, of vast extent, and of a quality far superior to any known deposit, if I could support this statement by the testimony of eminent mining engineers, if I should submit to you samples of iron and steel manufactured from this ore, and should produce vouchers from experts as to their superior excellence; and if I should further announce that the Baldwin Locomotive Works and the Phœnix Iron Works were ready to accept the product of this ore for their supplies,—I think there is very little doubt but that sufficient capital could be found here in Philadelphia to develop the enterprise. In accepting your invitation to speak before your Board of Trade this evening, I desire to present a subject of far greater importance to the material interests of this city and State than all the mineral wealth of the district; and if I cannot justify liberal expenditures in promoting it on grounds as substantial, tangible and practical as can be urged for investment in any of the mining enterprises of the State, I shall have entirely failed in my argument.

Before entering into the details of the discussion I wish to say a word or two in explanation of the term value, a term which I shall have frequent occasion to use. In a commercial sense, value means the price or worth of the thing bought or sold; in economic science—setting aside for the time being any discussion of the effect of supply and demand upon value—the value of an article may be said to depend upon the original value of the raw material and the value of the labor which has been expended upon it. And, further, the original value of the raw material is usually so slight that we may say, speaking broadly, that any particular value set upon an article, such as $1 or $10, means that an equivalent amount of human labor has been, as it were, detached from the indi-

vidual, concreted and put into definite shape for exchangeable purposes. One step further: human labor is but the expression of human thought. From the rudest kind of labor—labor which expresses hardly more than the force of mere animal push or pull—to the labor of the designer, the artist or the poet, all is but the expression of that wonderful human force which we call thought. Thus much being true, we reach the conclusion that human thought, as expressed by human labor, constitutes the principal factor in all values. As the full discussion of this statement will lead us to some important educational, commercial, and political considerations, I desire to make the point perfectly clear, and therefore invite your attention to a few illustrations of the manner in which thought creates values.

I hold in my hand a piece of steel. Its value is perhaps five cents. As yet it may be said to represent hardly more than so much raw material. In this hand I hold another piece of steel, of a similar quality, but less in quantity, and yet this latter has a value of twenty dollars. What makes this difference in value? Simply this, that human thought has been playing, as it were, about this latter piece of steel, and has made it the basis upon which it has concreted itself, so that we have an instrument of great practical use, a micrometer caliper—mainly the product of thought as expressed by skilled labor. Take this copper lamp. Here, again, we have perhaps ten cents' worth of raw material, carrying a value of five dollars, four dollars and ninety cents of which expresses the value which has been created by thought or skilled labor. In this porcelain vase observe how an insignificant value of raw material is made to carry a still greater value, created by thought. In this instance, we have a few cents' worth of clay transformed by skilled labor into a work of art, and carrying a value of over fifty dollars. Again, in these pieces of cotton goods, and in these Hamburg edgings, we have a few cents' worth of raw material, cotton, carrying values, created by skilled labor, a thousand fold greater than the value of the raw material itself. If we examine industrial products in any department, we find the same condition of things holds true—that the main value in the things made is the product of human thought.

As an extreme illustration of the great value of thought which iron and steel can convey let me quote the following calculation made by Dr. George Woods, of Pittsburg:

Seventy-five cents' worth of iron ore may be made into:

Bar iron, worth . . .	$5 00
Horse shoes, worth . .	10 50
Table knives, worth . .	180 00
Fine needles, worth . .	6,800 00
Shirt buttons, worth . .	29,480 00
Watch springs, worth . .	200,000 00
Hair sprins, worth . .	400,000 00
Pallet arbors, worth . .	2,577,595 00

It is also to be noted in this connection that thought has made itself felt in commerce in the production of articles of beauty as well as of use. This vase, whose absolute utility is no greater than that of a common earthenware pitcher, is rated in the market at a value many times that of a vase of ordinary form and color—and the value of beauty, which thought has created here, is just as important, commercially, in these days of our higher and more exacting civilization, as the value of use which thought has perfected in this micrometer caliper. We reach the conclusion, therefore, that in industrial articles, whether they be for use or for beauty, it is the value of the thought expended upon them which principally determines their commercial value. And if we extend our observations, we see that this state of things holds true all about us. The room in which we are assembled, the building of which it forms a part, the contents of this building—what has given to them the value which they represent? Certainly not the raw materials of which they are composed; rather it is the thought which the raw materials are carrying. Step out into your streets, observe the contents of your warehouses and your stores. You find thought expressed in finished products, and you find also food and materials on their way to serve as a supply to this thought as it labors in the expression of its ultimate purpose. Look further. In your machinery and wonderful mechanical contrivances you have this same human thought attaching itself to the elemental forces of the universe, and subduing steam, electricity, magnetism, to the common service of man. Now, it is human thought acting through human labor, and vivifying it with various degrees of intelligence and skill, that is accomplishing these myriad works, and creating these myriad values.

With these illustrations before us, and with this survey of objects which surround our daily lives, we must see plainly that there are two factors which enter into whatever is produced by human hands—raw

materials and the thought which has made use of these raw materials. And we see, further, that of these two factors, thought is by far the more important, material being simply the foundation, or the basis, upon which thought displays itself. Bearing these facts in mind, if we look at the enormous industrial forces which are gathering in England, France and the United States, and observe how human thought is displaying itself in these activities, we may, perhaps, realize something of the commercial changes which are taking place about us, and of the more important ones which are impending in the near future; and we may reach some appreciation of what thought really means as an article of commerce, and of the effect which the development of human thought in industrial directions is likely to produce, morally, socially and politically.

Take England, for example. The careful student of modern English history must look to the trade and commerce of Great Britain for the true explanation of the great political questions which now agitate the English people; indeed, the Irish question, and the still greater question of land tenure in England, which lies behind it, are matters which will ultimately be adjusted by the commerce of Great Britain, and it is the industrial element in this commerce upon which the other elements mainly depend. England will be able to hold her own and to overcome the many forces which are now setting against her only so long as she can maintain the industrial supremacy which she has secured by selling the thought of her people. When her manufactures fail, she will fail irretrievably in her commerce, and many of her institutions will share in that ruin.

Although England is perhaps the most striking example of national dependence upon the concretion of human thought, we find it to be true in all civilized countries of the first rank, that the elements which go to make up national power are centering more and more around the industrial forces. It is upon these industrial forces that the leading States now rely for the maintenance of their political as well as their commercial supremacy; it is through the immense activity of the industries that the four quarters of the globe are being probed for food and raw materials, that methods of transportation and distribution have been developed, and that the huge manufacturing centres which play so great a part in modern political economy have reached their present importance. Industrial development has become, indeed, a profound national question. Under these circumstances it is not out

of place for us to study with much care the industrial development of our own country.

For the purpose of bringing the matter concisely before your minds, let me invite your attention to a few commercial statistics in regard to the relative wealth which England, France, and the United States are creating by the concretion of human thought in industrial labor, and the bearing which this wealth has upon commerce:

ENGLAND.

	Imports.	Exports.
Raw material, . . .	$784,236,980	$117,727 03
Food, . . .	885,086,960	33,217 32
Manufactures, . . .	174,894,340	854,093 19

FRANCE.

	Imports.	Exports.
Raw material, . . .	$425,320,200	$250,838,600
Food, . . .	364,721,800	
Manufactures, . . .	84,183,600	347,098,200

UNITED STATES.

	Imports.	Exports.
Raw material, . . .	$182,057,686	$310,900,287
Food, . . .	215,219,419	439,996,838
Manufactures, . . .	247,065,378	73,081,365

It will be observed in the exhibits of England and of France that it is the manufactures which give life to the commerce of each nation, consuming food and raw material on the one hand as imports, and exchanging manufactured products on the other, as exports.

An examination of these statistics shows us that in the markets of the world England and France are great sellers of the thought of their people, while the United States sells but $73,081,365 of thought, and buys foreign thought to the amount of $247,065,378. It is true that the industrial power of the United States appears at some disadvantage in this exhibit, owing to the fact that most of our manufactures are consumed in our home market; but while this table more than suggests the industrial advantages which we have over England and France, by reason of our supplies of food and raw materials, it also makes painfully evident the fact that, in proportion to our opportunities, we are far behind the other two nations in the extent and variety of our industrial development.

Our inferiority in this direction was everywhere noticeable at the Centennial. I remember having been called to the Exhibition in March, 1876, before the opening, and meeting in the Main Building the manager of a New Jersey pottery. He was jubilant over the glories of his exhibit, and the certainty which he felt of securing a high award. Being at the building a month later I met this New Jersey potter again. He was standing before the English and the French exhibits of porcelain and pottery then just uncovered. I found he had experienced a complete change of heart. Frankly acknowledging himself beaten, he said, "I am going home to learn."

The Centennial is an old story, but it taught us many valuable lessons. What could be more suggestive to the thoughtful man than the general display made by Great Britain and her Colonies? Those who recall this exhibit will remember how, on coming from the western entrance of the Main Building, we first came in contact with the raw materials from the Colonies, and that Australia was conspicuous by its exhibit of a monolith showing in bulk the extent of its gold product. Every step forward was the putting behind of these raw materials and meeting a higher degree of thought and skill; until in the grand transept, in the exhibits of the Doultons and the Mintons, we found the very soil of England transformed by skilled labor, and bearing values far greater than that of the gold of Australia; while in the Elkington exhibit gold and silver themselves became raw materials on which to float thought expressed by skilled labor more valuable still.

At this Exhibition we were forced to recognize the industrial superiority of France as well. I am told that the poorest grades of cotton in the French exhibit surpassed the very finest cotton products which we had to show. It may be said that the development of our textile industries has only just begun.

Acknowledging, then, the general inferiority of our industrial development, as we must, save in a few directions, and acknowledging also the importance of industrial power to any great nation, the question arises, How can our industrial manufactures be promoted? I answer, *By the promotion of that factor which is the main element of value in them, human thought expressed by skilled labor.*

The nature of the thought required, and the manner of expressing it, are indicated by the industrial articles which we have been examining. Take this caliper, for instance. As we have seen, but a few

cents' worth of iron ore is here carrying a value, created by thought, four hundred fold beyond the value of the material. Now, in order to render the raw material—iron ore—suitable for this ultimate purpose, thought has played all about it, has brought great natural forces to bear upon its constituent atoms, and has recombined its inherent forces in a manner suitable to sustain the idea that thought has desired to impress into and upon it. Again, in the case of this vase, thought has been, as it were, playing about the raw material of common clay, readjusting its constituent elements to serve an industrial purpose, and the material, when thus reconstructed, has become the basis, as it were, upon which thought has expended itself in a purely æsthetic direction, creating great value by ministering to æsthetic tastes. Again, in this copper vessel we have thought working through skill; and, without changing at all the constituent elements of the material, it has created a new value by impressing thought upon the metal by the skillful work of the hand.

Thus we see that thought, to create these wide-spread industrial values, needs to be enriched by science, which shall inform it with regard to the constituent elements of the materials it has to deal with, and the nature ot the chemical and physical forces which may be brought to bear upon them; and also by æsthetic art, that it may make use of the principles which govern the production of beauty. Furthermore, it requires the aid of graphic art, as a definite language in which to express its conceptions, and a knowledge of the manual arts, that there may be sufficient skill of hand to embody in the desired materials the thought expressed by graphic art.

Such is an outline of what is necessary for the development, enrichment and application of thought for industrial purposes. To elaborate the details of each subject is impossible within the present limits, and only the general features of each can be referred to here. Grouping them under the three heads of Science, Art, and the Manual Arts, the fundamental elements in each subject may be outlined and illustrated as follows:

First. In Science, a knowledge derived from practical observations of the laws and phenomena of chemical and physical science, such as light, heat, electricity, magnetism, mechanics, molecular motion, chemical affinities, quantitative and qualitative analysis, etc., etc.

These are some of the more fundamental features, and need not be more than referred to here.

Second. In Art, a knowledge of the features of graphic and æsthetic art, which are three fold in their nature and relate to

1. Construction, or how industrial objects are made.

2. Representation, or how objects appear.

3. Decoration, or the enrichment of articles by ornament for the purpose of increasing their value.*

Third. In the Manual arts a knowledge of the fundamental manipulative processes in dealing with raw materials. The worker in iron for example, must be familiar with the processes of bending, drawing out, welding, punching, etc.; and in wood work a knowledge of planing, cutting and splitting, sawing, joining, turning, etc., is essential. In steel work we have the following, among various manipulations: Filing to line; sawing and filing; free-hand filing; fitting; chipping, etc.

There are various industries whose manipulative processes might be educationally arranged in a similar way.

Having thus seen that in industrial products the most important and most valuable factor is human thought, having seen further that for the practical development of this thought it needs to be enriched by a knowledge of Science and Art, and equipped with a knowledge of the Manual arts, our next inquiry is, How can these results be secured?

Before answering the question directly—and especially as by our statistics we have brought the commerce of England and France into contrast with our own—let us turn our attention for a moment to what England and France, our two most powerful industrial and commercial competitors, are doing in the way of protecting their commerce by protecting and stimulating their industries.

A brief survey of their efforts in this direction will show us that to an extent far beyond what is comprehended in this country they are utilizing the forces of public education as tributary to this end. Our statistics show us that England is leading the whole world in commerce, and this through the industrial development which is carrying her commerce along with it. If we look carefully into the imperial

* These features were illustrated by a number of charts and drawings, showing how drawing is practically applied in industry.

The work in iron, wood, and steel was illustrated by examples of shop work from the Institute of Technology, Boston. The teaching of special or complete trades in this elementary construction was discountenanced.

policy of Great Britain, we shall find that this result is no mere accident, neither is it wholly the result of fortuitous circumstances.

It is a result which has been deliberately planned. For more than thirty years England has been spending immense sums of money for the avowed purpose of developing her industries through increased skill and taste. So systematic have been her efforts, so munificent her expenditures, that she has established industrial museums and schools of science and art by the side of every important industry in the kingdom, and has expended over $20,000,000 in supporting them; and she is now supplementing these efforts by still broader provisions for the industrial education of her artisans in her national schools.

It is generally conceded that it is to these efforts that England mainly owes her present industrial supremacy.

If we turn to France, we find similar efforts in progress. In many respects the higher branches of technical education are better systematized and better developed in France than in England. At the same time, the trade education in France—that is, the training in the trades themselves—is broader than in England.

The recent efforts of England, however, through her national schools, will soon place her still further in advance. It may be said in this connection that France is endeavoring to meet England on her own ground, as will be seen by an examination of the recent provisions for public education in France.

If we were to extend our observations to Germany and Austria, we should find in those countries, also, the evidences of an earnest industrial arming. Indeed, Austria, a State which thirty years ago was behind nearly all Europe in education, is now looking to the development of industrial education with a liberality and a degree of practical foresight which challenge our most serious consideration.

To measure the exact effect in dollars and cents of such educational investments is, of course, impossible; yet it is the concurrent testimony of all who have given attention to the subject, that by such means only can industry be promoted in any broad way, while the results which have flowed from the efforts which have been undertaken, particularly in England and France, have been simply incalculable. If time permitted a study of the details of English and French commerce for the past few years it would be full of interest in this connection.

These facts show us that for industrial development—for the pro-

motion of thought in industry—public education is a most powerful weapon, and our commercial statistics tell us only too plainly that we in this country, with the most opulent material resources, and with a most intelligent and enterprising people, have hardly begun a course of industrial development.

For the purpose of substantiating the statement made in the beginning of these remarks, permit me to turn aside for a moment from the main argument, to bring this matter directly home to you here in Philadelphia.

I am assured that the annual product of your local manufactures is about $500,000,000. If this be so, you have an immense interest at stake in this question, and if what I gather from some of your leading manufacturers be true, your industries are far from having reached their maximum development in the matters of skill and taste. This being true, the assistance they need, the *vital protection* they need, is to have more thought in them—a higher degree of skill and taste among the artisans engaged in them. By providing this thought and this skill they will be most securely protected, and I do not think it too much to say, with the results of European experience before us, that the expenditure of $1,000,000 annually for broad industrial education would soon return you an increase of ten times that sum, in the enhanced value of your products, without additional cost for material, and with a reduction of cost in many directions. With such facts before you, with your vast industrial interests at stake, with the great activity of your industrial competitors on the other side of the water, aiming their industrial forces directly at your workshops, deem me not impertinent if I ask, What are you doing in the way of practically protecting your most important interests? Where are your schools of Science and Art, and your Industrial schools for artisans, such as you find at Birmingham, Sheffield, Leeds, Kommatow, Mulhouse, Creusot, Zurich; or your Industrial museums like those at London, Paris and Vienna?

This is a question for you to answer for yourselves. Without going into further details, you will agree with me, from what has been shown, that not only does your future industrial prosperity depend upon your securing more thought as expressed by skill and taste in your industries, but also that one of the most important matters which can engage the serious intention of your community and Board of Trade is this question of practical industrial education.

Turning now to the broader, or to the national aspect of the subject, the question arises, how can the education which we have seen to be so valuable, and which is being so earnestly promoted abroad, be introduced in this country?

I answer, it can be introduced in this country better and far more effectually than in any other country. We have simply to avail ourselves of our already established system of public education. By a readjustment of some of its features, and an incorporation therein of certain others, the main object can be accomplished. In this matter I know there are some difficulties in the way, and many prejudices to be overcome; still the thing can be done, and nothing is surer than the fact that the public schools will ultimately be made to answer to broad public needs in this respect.

In engrafting industrial education upon our public education we shall have a great advantage over our foreign competitors in this respect. Our general system of public instruction has already been organized. With all its drawbacks, it is free from many of the bones of contention which surround public education abroad. On the other hand, in promoting industrial development, the public schools will be advancing their own interests. They have of late received the fiercest criticism from many quarters, and the burden of this criticism seems to be that our public instruction is wanting in practical elements. But I cannot state this aspect of the case more strongly than it was stated in an address before the Franklin Institute in 1874, by one of its most honored members, Mr. Coleman Sellers, who summed up his argument by saying, "Our common school education gives us traders, gives us shopkeepers, but it gives us no artisans. I know not if this can be remedied, but I do know we require some other training for our sons and our daughters."

This feeling is so universal a one, and is so closely related to the subject in hand, that I trust you will pardon me for stopping a moment to show, in a graphic way, why it is that our public education is so practically out of joint.

In all education what is it that we educate? Where do we lodge the instruction? What particular organs, senses, faculties, do we develop? The effort is to develop thought. What is thought, and how is it generated? Thought is brain power. Thought in literature, commerce or industry is produced by the legitimate physical action of the brain, and to study the purely materialistic action of the brain

in generating and expressing thought becomes a primary consideration in all education.

Studying the functions of the brain, we find that for educational purposes it may be likened to an organism with a threefold form of working, an organism with a power of absorption, a power of assimilation and recreation, and a power of expressing or giving out. The force or character of a brain is measured entirely by its expressing power, by what comes out of it. Examining a little closer, we find that the brain absorbs through all the five senses, while for expressing purposes it makes use of but two of these senses, or rather of but two organs of these senses—the tongue and the hand. Fig. 1 is a simple

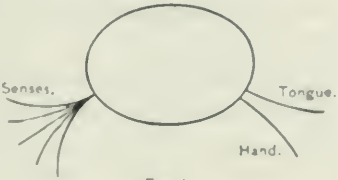


Fig. 1.

diagram representing a brain with the five senses placed on one side, as means of absorbing power, while on the other side the tongue and the hand are placed as organs of expressing power. The other function of the brain, that of assimilation and recreation, cannot of course be graphically represented. It may, however, be said to be the result of the action of the other two functions. Now the equipping of a brain, or the healthy education of a brain, consists in giving it expressing power through the tongue and the hand, coextensive with the power of absorption and the power of recreation.

Applying our popular schemes of education to the brain, and especially those based on the 3-R idea of education, we find what is indicated in Fig. 2, that provision has been made for greatly distending

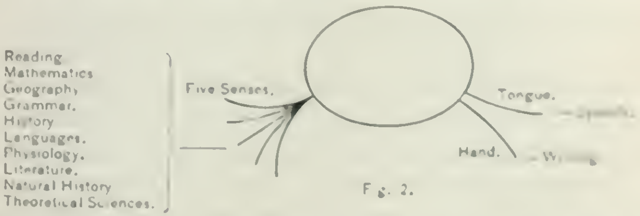


Fig. 2.

the absorbing side of the brain, while for the expressing side, the practical side, provision has been limited to the use of the tongue in

speech and to the hand in writing. If now we follow the result of this brain equipment into practical life, we find that speech and writing as means for expressing thought have their applications mainly in the commercial and financial employments and the professions, and only incidentally in the industrial and mechanical employments. With such an inadequate and one-sided brain equipment, it is not possible in any broad, practical way to bring thought or brain power to the service of industry. The fact so generally admitted that we are getting so few intelligent artisans or mechanics from our scheme of public education, that we turn out pupils of both sexes with a decided repugnance to industrial labor, is an attestation to the truth of this statement. The simple fact is, that our education is not broad enough on the expressing side of the brain, that too much attention has been given to the absorbing side of this organ, that no adequate provisions have been made whereby it can discharge its power in work connected with the industries.

In Fig. 3 a remedy for this defect is indicated in the addition of

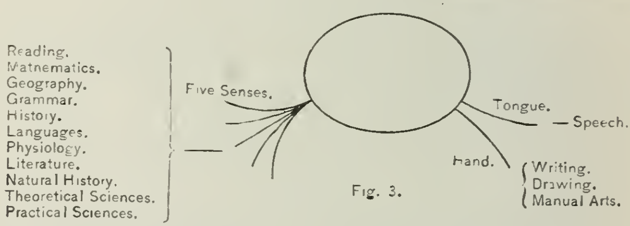


Fig. 3.

the study of graphic and æsthetic art, through drawing, and of training in the manual arts, to the previous brain equipment. Observe where these features come in the scheme—on the expressing side of the brain and in the service of the hand, thus giving the brain ample power to discharge thought in its most complete form for use or for beauty. With these features added to the brain equipment its power of expressing thought in all practical directions will be coextensive with its absorbing and recreating powers; and just as soon as the public can clearly see that in the outcome of our public education there is no respecting of persons or of classes, that pupils are trained for honest labor with their hands as well as to living by their wits, are taught to produce something, to *create values* by the action of their brain through the work of their hands, a much deeper interest in public education will not only be manifested, but generous provisions for its support will also be given.

The practical solution of the problem we have been considering is not a difficult matter. It consists of providing better instruction in Science and in Art, and in making provisions for the instruction of our youth in some of the Manual arts.

All that is necessary in the instruction in Science and in Art is to reform the teaching in both subjects, and to make it more practical. Bring Science down from the High School to the Primary and Grammar Schools: teach it not by theory, nor even by the ordinary object lesson, but give pupils an opportunity to work with materials, to make experiments, and thus to observe the results which take place under their own fingers, thereby gaining knowledge as well as manipulative confidence and skill. All this is possible.

In teaching Art a radical reform should be instituted. Substitute for the dilettante drawing which cumbers many of our schools, systematized and practical work like that which we have been considering, and which finds practical applications in education in our industries and in true æsthetic culture.

In regard to training in the Manual arts, the question being one of comparatively recent growth presents more serious difficulty. The principal lines of work have been suggested and illustrated already; but that which has been accomplished in this direction has been done mainly in special mechanical schools like those of the Boston Institute of Technology, or of the Washington University at St. Louis, and a few other technical schools. The present problem is to bridge over the ground between the industrial training of the Kindergarten and that of these technical schools.

I think, however, that a full consideration of the question will show no insurmountable obstacle in the way of attaching like training schools to our public school system, or in the way of ultimately incorporating instruction in some of the fundamental manipulations in the Manual arts into a regular scheme of public instruction. Various efforts are being made to solve this problem, and with the general interest in the subject which now exists, I have no doubt but that the demands of practical public education will make themselves felt in this direction.

If my argument, then, be a sound one, we see that the material prosperity of this people, and especially of this community, is coming to depend to a very great extent upon the development among all classes of a knowledge of industrial science, art, and manual skill; that the promotion of this knowledge is fundamental to any broad material

development, and that as yet but very meagre provisions have been made for it.

We see also that the most efficient provisions which can be made for this education must be made in connection with the public schools; and that in order to give proper elementary industrial training in our schools, we must have more practical and universal teaching in Science, and more carefully systematized, more definite and broader instruction in industrial art; and that training schools in the Manual arts should be grafted upon our present system of school instruction.

These ideas are not new; many of them have been carried into practical effect, as we have seen from our illustrations; but although the subject is not a new one, it is one whose educational, whose commercial, whose political importance is now for the first time gaining recognition. The more careful the study given to this question, the more vital does the necessity for this development of industrial thought appear, and I do not feel as if I could emphasize too strongly the imperative necessity to our political and commercial prosperity of a judicious investment in this tremendous, this incalculable source of wealth, the development of the industrial thought of the nation.

A serious word just here. We hear much in these days about protection to American industry. If my presentation be a true one, we are to-day sadly unprotected against the industrial competition that is coming against us from Europe, a competition against which no tariff can alone protect us. Remember that it is not now the pauper labor, but the skilled labor of Europe with which we have to contend. Europe has been arming for this contest for years, and is now putting millions of skilled workmen into her workshops. Against such a competition no tariff can give adequate protection, and if I may be permitted to do so, I would suggest as a fundamental article in the creed of protection, *The industrial education of the American workman.*

One word more in explanation. I am aware that in stating the question thus broadly and practically, I lay myself open to the charge of advocating a materialistic education. Such, however, is not at all the result which I believe will follow. In a government like ours the development of good citizenship must always hold a foremost place in all schemes of education.

What is the basis of good citizenship?

It rests, in individual cases, primarily on the power of maintaining

oneself in the struggle for existence; and when you observe the complex conditions under which life is given to us to-day when you consider the necessity which rests upon every mechanic and artisan of Philadelphia that he shall produce something by the action of his brain and hand, something which shall exchange with the food produced by the Illinois farmer on the one hand, and with the work of artisans or producers in other communities on the other, you see that the first condition for good citizenship on the part of your industrial producers here in Philadelphia is the possession of the power of supporting themselves by selling the expressions of their thoughts in iron or wood or steel or textiles. If it be not too abstract a thought, it may be said in this connection that the degree in which man becomes a good citizen, and the degree in which he becomes interested in the whole scheme of social and political order, can be approximately measured by the means afforded him for the creation of wealth and for the exchange of his products.

It may be alleged that I put even a material value upon citizenship. In one sense, I do. The material value of social and political order should, it seems to me, be the starting point for the consideration of the subject, for, with all due respect to theoretic sentiment, in these days when the rapid increase and aggregation of population in industrial and commercial centres are presenting new problems in social, political and economic science, the responsibilities of life are too real and too fearful to admit of our relying wholly upon theories of human conduct, however sacred or however venerable. Strikes among miners and trade-union operatives are the legitimate, wasteful and barbarous attendants of our present industrial development. These evils are not to be corrected by the establishment of Sunday schools or by the distribution of religious tracts. We have first to deal with the people engaged in the industrial employments as men, having like interests, hopes and fears with ourselves. We must give those who live by the industries a fair chance; we must afford the men who are engaged in industrial labor an opportunity to live as men, and not as unthinking machines, or as ignorant beasts; and the first great step towards the reconciliation of labor and capital seems to me to be this,—industrial education.

No, it would be an entirely false conclusion to say that the idea of education we have been considering tends to merely materialistic ends. It is true that its first object is to reach a material result through the

concretion of human thought, either for use or for beauty. In the process, however, the very foundations of human knowledge, we may say of human culture, are laid hold of. The mind of man is pressed by an inexorable necessity against the primal forces of nature, feeling "God's great hand in that darkness;" and in studying the constituent action of these forces, in learning to appreciate their infinite extent, their marvelous power, he is brought into direct contact with eternal things. Whether dealing with iron, or textiles, or precious metals, he comes at last to see that these very materials in their last resolution are but emanations of that supreme power, which, clothed though it may be in the phraseology of the "Persistence of force," or invested with the personality of Zeus or Jehovah, pervades all things.

Industrial education therefore, properly comprehended, means not simply the training for a trade, but the building up of a good citizen, who contributes by his thought, expressed by skilled labor, to the happiness of mankind. Trade and commerce are but servants to such a citizen, and by exchanging his products, they link him indissolubly to the preservation of social and political order, as mere accessories to the full development of his own spiritual individuality.

And this is industrial education from a business standpoint.

Variations of the Hydrogen Lines.—Rutherfurd first established a classification of stars according to the nature of their spectra by pointing out the differences in the hydrogen lines in different stars. Secchi confirmed his observations and showed that the solar lines, especially those of sodium, magnesium and hydrogen, vary in breadth and intensity in the neighborhood of Sun spots. The late beautiful researches of Huggins have also shown variations, in the violet and ultra-violet portions of the spectrum, which can only be studied by the help of photographic processes. Fievez finds, by experiment, that the breadth of the lines varies in proportion to the elevation of temperature, and that very slight traces of one or more elements can be detected by a sufficient elevation of temperature. In this way he finds that the C and F lines of hydrogen are each bordered by two fine lines, which form a triplet with the principal lines. He regards his observations as interesting, in view of the study of the harmonic or rhythmic structure of spectral lines, which has been pointed out in this Journal (vol. civ, p. 288, etc.)—*Bull de l'Acad. de Belg.* C.

Fluctuations of Sulphur Springs.—M. Villot has studied the variations in the flow and strength of the sulphur springs at Camoins-les-Bains. The waters are cold, and their sulphur appears to come from the decomposition of gypsum by the organic matters with which the surface waters become charged while circulating upon the surface or in the sub-soil. They contain free sulphuretted hydrogen, together with sulphuret and sulphate of lime in solution. Irrigations by artificial canals injure the spring, sometimes diminishing the strength of the waters more than two-thirds when the bathing season is at its height.—*Ann. des Mines.* C.

Cotton in Italy.—The American war stimulated the cultivation of cotton in Italy, and excited great expectations of permanent prosperity. The continual and sudden diminutions of temperature, during the season when the bolls are ripening, proved a great obstacle to the cultivation in the southern provinces, and it is now confined almost entirely to limited districts in Sicily and the lower peninsula. The factories and private looms for weaving textile fabrics suffer greatly from competition with the importations from other countries, and the question of a protective tariff is awakening much interest. Fedele Borghi, referring to the early history of cotton manufacturing in the United States and to its subsequent marvelous growth, believes that a similar protective policy would lead to similar results in Italy.—*Il Politecnico.* C.

Modification of the Ruhmkorff Coil.—G. Scarpa and L. Baldo constructed a Ruhmkorff coil with a helix divided into three movable compartments. Under the ordinary arrangement the sparks produced by three Bunsen cells had a length of 6 centimetres (2·36 in.); when the middle compartment was removed without disturbing the others the length of the spark was perceptibly increased; when the thread of the middle compartment was wound around that of the other compartments they obtained a spark of 8 centimetres (3·15 in.) They then wound the wire so that the current would pass from the exterior extremity of one of the superficial coils to the corresponding central coil, and then continued through the other coil to the exterior extremity of the second superficial coil. With this arrangement they obtained a zig-zag spark of 13 centimetres (5·12 in.), of exceeding brilliancy.—*Revista Scientif. Indust.* C.

Lunar Observations in Algiers.—M. Faye has communicated to the French Academy the first series of lunar observations which were made at the new observatory at Algiers by its director, M. Trépied. These observations strikingly confirm the accuracy and importance of Newcomb's corrections of Hansen's tables. M. Faye anticipates from a continuation of the observations further valuable results, which will amply justify the establishment of the new observatory.—*Comptes Rendus.* C.

Electricity of Mechanical Vibrations.—Mousson, in 1858, found that the conductibility of metallic wires is affected by vibrations. Dr. de Marchi has recently resumed the investigation. He finds that every stretching of a metallic wire generally increases its resistance; the increase is commonly proportional to the increase of stretching, but after reaching a certain limit the variations proceed by jerks, showing a momentary and profound disturbance of the molecular state of the wire.—*Revist. Scientif. Indust.*

[Chase showed in 1864 that the variations of the magnetic needle could be explained by the mechanical action of the convection currents of the atmosphere.] C.

Relation of Magnetism to Atomic Weight.—Leo Errera deposited with the Royal Academy of Belgium, in January, 1878, a sealed note upon the "law of magnetic properties," which has lately been published on account of Carnelly's having arrived at conclusions identical with his own. He concludes that the bodies of the uneven series, in Mendelejeff's groupings, are diamagnetic; the bodies of even series are paramagnetic. He proposes to show, in subsequent communications, that the periodicity exists not only in the fact of paramagnetism or diamagnetism, but also in the intensity of those forces. He is now endeavoring to establish a like periodicity for many other physical properties, especially for the points of fusion and volatilization. He predicts that vanadium will be found decidedly paramagnetic; lithium, rubidium, zirconium, rutherium, cœsium, thorium, and perhaps ytrium and erbium, will be slightly so. As to calcium, strontium and barium, contrary to the opinion of Faraday, they will also probably be attracted, at least slightly, by a very powerful magnet. On the other hand, thallium, gallium, and probably indium, will be diamagnetic.—*Bull. de l'Acad. de Belg.* C.

Curious Physical Phenomenon.—Dr. Grassi used three concentric vessels with an annular space of about 2 centimetres (·787 in.) between the first and second and between the second and third. The outer space was filled with oil and the next with water. The oil was heated by a furnace to a little over 100° (212° F.), and the water boiled. Then hot oil, *e. g.*, 150° (302° F.) was poured into the central space. This quickly cooled to a temperature about equivalent to that of boiling water. The oil cooled more rapidly the higher the temperature of the outer oil. Dr. Grassi referred to an analogous phenomenon, discovered by some members of the Academia del Cimento, who found that the water in a vessel surrounded by ice cools more rapidly when the ice is heated in order to accelerate fusion.—*Proc. Roy. Inst. Lomb.* C.

Prevention of Locomotive Sparks.—Albert Focke proposes the following arrangement for preventing the escape of sparks from the chimneys of locomotives. A conical tube is fixed centrally within the chimney, with its large end extending down just into the blast pipe and its small end reaching about half way up the chimney. Above this conical tube is fixed another and shorter tube, in the form of an inverted truncated cone, above the mouth of which there is a metal disc. Part of the exhaust steam which issues from the blast pipe passes up through the lower conical tube into the upper one, and is deflected by the disc, outwards and downwards, into the annular space between the tube and chimney. Sparks that are carried up the annular space by the exhaust steam from the blast pipe are met by the counter current and are thereby extinguished.—*L'Ingen. Univ.* C.

New Electric Property of Selenium.—Blondlot attaches to one of the poles of a capillary electrometer, by means of a platinum wire, a fragment of annealed selenium; to the other a plate of platinum. If the selenium is brought in contact with the platinum, by an insulating rod, the electrometer remains at zero; but if the selenium is rubbed against the surface of the metal the electrometer is immediately powerfully agitated. A deviation may thus easily be obtained, equivalent to that which would be produced by a cell of sulphate of copper. Neither the friction of two metals against each other, nor that of two insulators, can produce any charge. The selenium current flows through the electrometer from the unrubbed to the rubbed selenium, consequently the disengagement of electricity cannot be attributed to the heat which accompanies the friction.—*Compt. Rend.* C.

Book Notices.

The Civil Engineer's Pocket-Book. By John C. Trautwine. Fifteenth thousand. 1881.

The question has frequently been asked, "What becomes of all the pins?" It might almost as well be asked in reference to this well-known and useful work—one which might be supposed to be used by the comparatively few only, but which, from the extensive demand for it, must be much more widely used by other technical men than civil engineers, for whom it was originally designed. The present edition—the second one this year—has been carefully revised and enlarged, errors of earlier editions, typographical and otherwise, corrected as far as noticed, although a few comparatively unimportant ones still remain.

Among the articles added that on "Centres for Arches" will be found a very useful and instructive one for engineers particularly the large number who have not had the practical experience of the veteran author, giving as it does examples of defective construction and modes of striking centres to be avoided, as well as examples of successful construction and proportions desirable to be followed.

In glancing over the tables, in that on page 416, "Table of Radii, Middle Ordinates, etc., of Curves," and incidentally comparing them with some calculations of our own, in the small angles below 10 minutes, we find some discrepancies. As these angles are hardly ever used, a correction would be needed only for strict accuracy; above 10 minutes there is no material difference as far as noticed.

As we learn that those in this work were computed from Hutton's tables of *natural* sines to seven places of decimals, while ours were found by using Young's *logarithmic* tables (carefully compared with those of Callet, Vega, etc.) to the same number of places of decimals, the discrepancies must have arisen from the use of the different tables from which the computations were made, so the question arises, which to use, natural or logarithmic tables for small angles? We prefer logarithms, as will be seen by the following.

Our author has used the following rule: Subtract angle of deflection from 180°, then say as natural sine of angle of deflection is to natural

sine of *half* the remainder so is the given *chord* to the radius required.

Example.—Let the angle of deflection be 0° 01′, chord 100 feet (as usual in the table), required the radius.

$$\overline{180° - 0° 01'} \div 2 = 89° 59' 30''$$

Then, as $\frac{\text{nat. sine } 0° 01'}{\cdot 0002909}$: nat. sine, $\frac{89°}{1}$ 59′ 30″ : : $\frac{\text{chord}}{100}$: $\frac{\text{radius}}{343760 \cdot 74}$

Our practice is to use logarithms according to the following formula: As the chord of the arc is 100 feet, in setting out stations on a curve the sine will be one-half that = 50 feet, and as the angle of deflection D is one-half the angle at the centre, known as the "degree of curve," we will have $\therefore R = \frac{50}{\text{sine } D}$.

Example by *logarithms* for 0° 01′ curve:

2) 0° 01′		10·
0° 00′ 30″	Log. Sine.	6·1626961
	Ar. Co.	3·8373039
50 feet.	Log.	1·6989700
343774·64 feet Radius		5·5362739

As the angle increases, the results by the two methods approach more closely until at 0° 10′ they nearly correspond.

As somewhat similar discrepancies were also noticed in some pocket table books by other authors, the following will be found useful for correction and comparison.

Angle of deflection.	By natural sines. Trautwine. Radii.	By logarithms. C. H. R. Radii.
0° 1′	343761	343774·64
2′	171880	171887·39
3′	114587	114591·55
4′	85940	85942·63
5′	68752	68754·93
6′	57293	57295·78
7′	49109	49110·68
8′	42970	42971·84
9′	38197	38197·19
10′	34378	34377·47
15′	22918	22918·33
6° 0′	955·4	955·366

Our author uses the terms "deflection distance" and "tangent distance," and we think much confusion would have been avoided if all the succeeding authorities had used the same terms to designate the same functions, as his were the first tables of the kind published, instead of which we find several different ones used by other authors.

We hope, at some future time, to follow up this subject in a more elaborate paper. C. H. R.

WOOD-WORKING TOOLS; HOW TO USE THEM. A Manual. 12mo. Boston: Ginn & Heath, for the Industrial School Association. 1881.

This neatly printed and cloth-bound book of about 100 profusely illustrated pages "aims to give in fourteen chapters directions and exercises for the use of the wood-working tools." While designed to aid an intelligent and practical teacher, its illustrations and careful analysis of the various positions, motions and operations should be of great use to amateur or apprentice. Its preparation seems attributable to Messrs. Channing Whitaker, Raymond D. Chapell, Alonzo W. Folsom, the late C. H. Dow, J. Phillips White, and R. L. Bridgman for the text; Frank Rowell and D. T. Kendrick having prepared the illustrations. As it is, the principal fault of the booklet is incompleteness; the excellence of the analysis of those operations which are explained in detail, causing regret that so many exercises are merely indicated, not carried out in detail.

The book would have been more accurate had it stated the difference required for saws, plane-bits and augers for hard, soft, clear and gummy woods, and modern carpenters would prefer to see a less ancient plane-stock than that chosen throughout.

Such books are useful, and this one in particular deserves wide sale and constant use among those for whom it is intended. R. G.

ERRATUM.—In the abstract of Mr. R. Grimshaw's paper on "Air Compressors," June number of the JOURNAL, in the paragraph referring to the Ingersoll Compressor, the words "vertical" and "closed with springs" should be omitted.

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EXPERIMENTS ON THE STRENGTH OF WROUGHT IRON AND STEEL AT HIGH TEMPERATURES.

By C. R. Roelker, Passed Assistant Engineer U. S. Navy.

In the following paper an account will be given of experiments made by Dr. Julius Kollmann in 1877–78 on the strength of wrought iron and Bessemer steel at high temperatures, and the results obtained by him will be compared with those obtained in previous investigations of the same subject.

Kollmann's experiments, described by him in a prize essay crowned by the "Verein zur Beförderung des Gewerbfleisses," 1878, were carried on at the iron works of the "Gutehoffnungshütte," at Oberhausen, Rhenish Prussia. Besides numerous tests of the tensile strength of iron and steel at temperatures ranging between 70 and 2000 degrees Fahrenheit, Kollmann's experiments comprised investigations of the resistance of these materials to compression in the process of rolling them into bars and rails, and of various other questions of great practical importance in the manufacture of rolled iron. The present paper will be limited to a consideration of the tensile tests.

Two machines, a larger and a smaller one, were used in making the tensile tests, so that each might check any error in the results obtained

with the other one; both gave, however, closely agreeing results. They were always carefully examined and adjusted before the commencement of each experiment. In the smaller machine the strain was applied to the test piece by means of a hydraulic pump and was measured by weights on a scale beam. In the larger machine the strain was produced by the direct application of weights to a beam, the stretch being taken up by means of gearing, operating upon a screw connected with the lower holder of the test piece.

For the smaller machine the test pieces were made of round and square bars, turned or filed exactly to the following dimensions, viz.: 0·59 inch square, or 0·51 inch diameter in the body for a length of 9 inches, their total length being 11 inches.

For the larger machine the test pieces were cut out of broad flat iron, the strain being always applied in the direction of the fibre. They were made 1·57 inches wide by 0 39 inch thick for a length of 1·38 inches or 2·56 inches, the shorter specimens being used for the higher temperatures, because the stretch of the longer specimens could not have been taken up in the machine.

The test pieces for the smaller machine were heated throughout their whole length in a coke fire. Great care was taken to heat the specimens evenly throughout. A duplicate specimen, having exactly the same shape and dimensions, was heated each time in the same manner to the same degree as nearly as could be judged from photometric measurements by means of colored glasses. This duplicate specimen was used for determining the temperature of the test piece.

The larger test pieces were heated in a forge over a length of 1·38 inches or 2·56 inches, the ends being kept cool by wet coal. A piece of iron having the same dimensions as the heated portion of the test piece was heated at the same time to the same degree, as nearly as could be judged, and was used for determining the temperature of the test piece.

The elongation of the test pieces under a strain was measured by means of an instrument called by Kollmann a "multiplicator." It consisted of two levers connected, like a pair of tongs, by a bolt which served as a common fulcrum. The proportion of the short and long arms of each lever was as one to ten. The ends of the short arms were provided with jaws fitted with set-screws, by means of which the instrument could be attached quickly to the heated test piece after it was secured in the testing machine. One of the long arms carried at

its extreme end an arc on which a scale was marked, and which passed through an opening in the end of the other long arm. When the scale of the arc marked zero, the centres of the jaws of the two short arms were 1·38 inches apart, and any increase of this distance, magnified tenfold, was easily read off on the scale.

The temperature of the test pieces was determined by means of a calorimeter, consisting of a well-insulated copper cylinder containing exactly two litres of water and provided with a cover to prevent loss of water by vaporization. The duplicate test piece, heated as described, was dropped into the calorimeter at the exact moment when the strain was applied to the test-piece. The water was stirred till the temperature of the whole mass and of the test piece was equalized, and the rise of temperature was measured by a carefully tested thermometer. The calorimetric values of the copper cylinder, of the stirrer and of the thermometer had been determined and reduced to an equivalent weight of water. The loss of heat by radiation from the calorimeter was found, by experiment, to be too insignificant to affect sensibly the results obtained. To find the temperature of the test pieces from the rise in temperature of the calorimeter a set of tables were used, which were published in the "Zeitschrift Deutscher Ingenieure," 1875. These tables are based on the following formula for the mean specific heat, C_m, of wrought iron between the temperatures T and t in degrees of the centigrade thermometer, viz.:

$$C_m = 0{\cdot}105907 + 0{\cdot}00003269[T+t] + 0{\cdot}0000000108[T^2+t^2+Tt]$$

The temperature of the test piece at the commencement of the experiment being thus found, it became necessary to determine how much it cooled off during the experiment. For this purpose numerous experiments were made with the two forms of specimens used in the testing machines. Their initial temperature and final temperature after exposure to the air for a known length of time were measured; the results thus obtained being plotted by laying down the lengths of the time of exposure as abscissæ and the corresponding temperatures as ordinates, a curve was obtained for each of the two forms of specimens, representing the law of cooling on exposure to the air. From these curves the final and mean temperatures of the specimens during the tests were deduced. In order to verify the results thus obtained, the final temperature was directly determined in a number of cases by dropping one of the broken parts of the test piece into the calorimeter.

The mean temperatures found in the above described manner were entered as the temperatures at which rupture took place. The rapid fall of the limit of elasticity and the great reduction of the cross area of the test pieces at high temperatures made it necessary to reduce the differences between the initial and final temperatures as much as possible, and for this reason the tests were made as rapidly as possible: the average duration of each test was but a little more than half a minute.

No allowance was made for any difference in the specific heats of wrought iron and Bessemer steel; the resulting errors in the determinations of temperatures, however, could be but trifling.

Three kinds of iron were tested, viz.: Fibrous wrought iron, fine-grained wrought iron, and Bessemer steel. All the test pieces were taken from the ordinary stock manufactured at the works of the "Gutehoffnungshütte," and not from specially selected material.

A chemical analysis of the specimens gave the following mean results, viz.:

	Fibrous Wrought iron.	Fine-grained Wrought iron.	Bessemer steel.
C, .	. 0·10	0·12	0·23
Si, .	0·09	0·11	0·30
P, .	. 0·34	0·20	0·09
S, .	0·03	trace	0·05
Mn, .	. 0·07	0·14	0·86
Cu, .	0·07	0·06	0·07
Fe, .	. 90·30	99·36	98·40
	100·00	99·99	100·00
Specific gravity,	. 7·62	7·69	7·84

The fracture of the *fibrous wrought iron* exhibited, as indicated by the name, a long fibre, but with traces of a coarse granular structure. The fracture of the *fine-grained iron* was rather fibrous than granular in appearance in consequence of the great reduction of the bars in the rolls. The *Bessemer steel* had a very even texture and a rather light shade of color.

The square and round iron bars for the small test pieces were rolled from single-piled hammered blooms. The broad flat bars for the large test pieces were rolled from double-piled hammered blooms. The steel bars were twice heated in rolling.

A number of tests made at ordinary temperatures with both machines gave closely agreeing results, showing that the materials were of a uniform quality. Each of these tests lasted from 5 to 14 minutes. The mean results of these experiments were as follows, viz.:

	Fibrous Wrought Iron.	Fine-grained Iron.	Bessemer Steel.
Modulus of rupture, in lbs per sq. in.,	52464	56892	84826
Limit of elasticity, " "	38280	39113	55029
Elongation after rupture, per cent.,	17·5	20·	14·5
Contraction of cross-section, per cent.,	20·2	23·77	22·2

Kollmann gives tables showing the results of experiments on the tensile strength of these materials at high temperatures, comprising 52 tests of fibrous wrought iron, 38 tests of fine-grained iron, and 37 tests of Bessemer steel, between the temperatures of 68 degrees and 1976 degrees Fahrenheit. He also gives tables showing the mean values of the modulus of rupture for each material, expressed in per centum of the modulus of rupture at ordinary atmospheric temperature, for each increase of 50 degrees centigrade in temperature from 0 to 1100 degrees centigrade, and graphical representations of these mean values.

	Kollmann's Experiments.			Franklin Institute Experiments.
Temperature in deg. Fahr.	Fibrous Wrought Iron.	Fine-grained Iron.	Bessemer Steel.	Wrought Iron Boiler Plates and Bars.
0	100·	100·	100·	96·
100	100·	100·	100·	102·
200	100·	100	100·	105·
300	97·	100·	100·	106·
400	95·5	100·	100·	106·
500	92·5	98·5	98·5	104·
600	88·5	95·5	92·	99·5
700	81·5	90·	68·	92·5
800	67·5	77·5	44·	75·5
900	44·5	51·5	36·5	53·5
1000	26·	36·	31·	36·
1100	20·	30·5	26·5	
1200	18·	28·	22·	
1300	16·5	23·	18·	
1400	13·5	19·	15·	
1050	10·	15·5	12·	
1600	7·	12·5	10·	
1700	5·5	10·5	8·5	
1800	4·5	8·5	7·5	
1900	3·5	7·	6·5	
2000	3·5	5·	5·	

The diagrams on the plate accompanying the present paper represent these mean values; the temperatures in degrees Fahrenheit being laid down as abscissæ, and the corresponding relative mean values of the modulus of rupture being represented by ordinates. In the above table the *second, third* and *fourth* columns contain the heights of these ordinates corresponding to the temperatures given in the *first* column, or, in other words, the relative mean values of the modulus of rupture in Kollmann's experiments.

Column *five* of the foregoing table contains the results of experiments carried on by a committee of the Franklin Institute in the years 1832–36, corrected as will be described further on. The same are represented graphically on the plate accompanying this paper, where the results of experiments conducted by Fairbairn, Styffe, and the British Admiralty are likewise plotted.

Comparing the plotted results of these experiments we observe the following facts, viz.:

1. In Kollmann's experiments the ultimate tensile strength remained unchanged till, in the case of fibrous wrought iron, a temperature of 200 degrees Fahrenheit, and in the case of fine-grained iron and steel a temperature of 400 degrees Fahrenheit was reached; and after these limits of temperature were passed, the ultimate tensile strength diminished rapidly. On the contrary, all the other experiments, with but a single exception, show a decided increase of tensile strength at temperatures higher than the ordinary atmospheric temperatures; this increase of strength differs greatly in quantity with different materials, and is frequently irregular, but it generally attains its highest value at temperatures lying between 400 and 500 degrees Fahrenheit.

2. At higher temperatures the ultimate tensile strength diminishes in a rapidly increasing ratio till, in the case of Bessemer steel, a temperature of about 775 degrees Fahrenheit, and in the case of wrought iron, a temperature of about 1000 degrees Fahrenheit is reached, when the decrease of strength continues in a greatly diminished ratio, which soon becomes approximately uniform and continues thus as far as experiments have been carried.

The curves representing this decrease of strength of different materials with increase of temperature, though diverging widely at many points, exhibit remarkable similarities in their general features.

It is to be remarked that Kollmann himself attaches the most importance to the results of his experiments at temperatures exceeding

…ought-Iron and Steel

…s.

…planation of Symbols

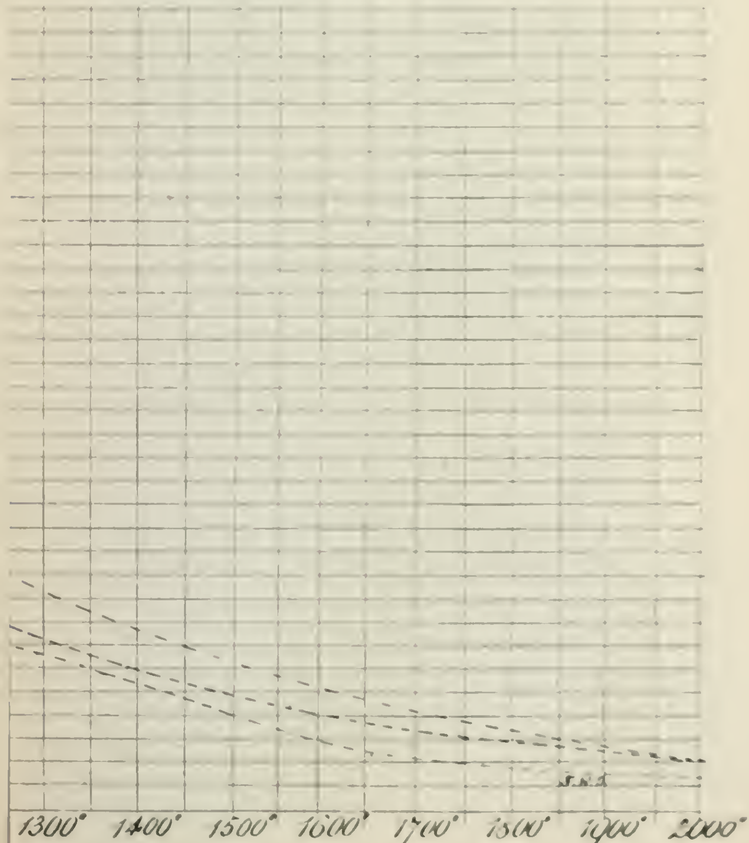
F.W.I. . . Fibrous Wrought Iron   Kollmann's Experiments
C.W.I. . . Homogeneous Iron . . .   "   "
B.S. . . . Bessemer Steel . . . . .   "   "

F.I.E.   Wrought Iron . . .   Franklin Institute Experiments

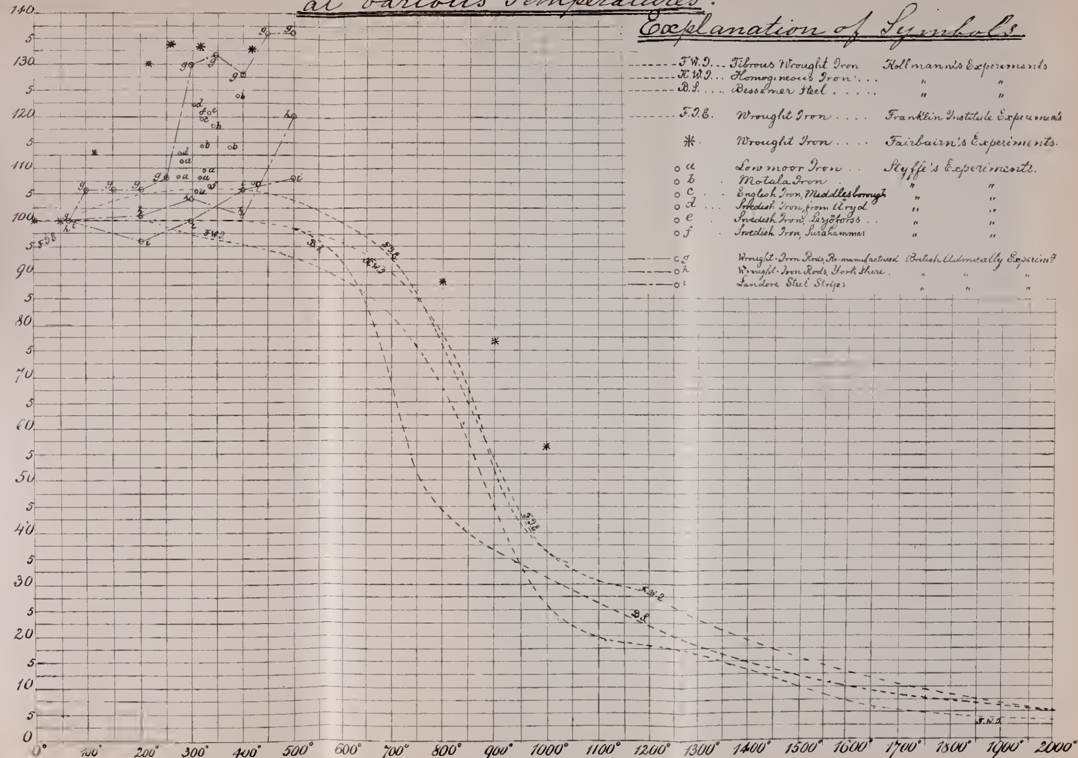
✳ . . .   Wrought Iron . . .   Fairbairn's Experiments.

a   .   Lowmoor Iron .   Styffe's Experiments.
b   .   Motala Iron .   "   "
c . . .   English Iron, Middlesborough   "   "
d. . . .   Swedish Iron, from Åryd   "   "
e . . . .   Swedish Iron, Lesjöforss . .   "   "
f . . . .   Swedish Iron, Surahammar   "   "

g   Wrought-Iron Rods, Re-manufactured   British Admiralty Experim^ts^
h   Wrought-Iron Rods, York Shire   "   "   "
i   Landore Steel Strips   "   "   "



Diagrams representing the Strength of Wrought-Iron and Steel at various Temperatures.



570 degrees Fahrenheit. The tests made below that temperature were few in number; but as they gave results showing a certain regularity, he considered them sufficiently trustworthy to use them in laying down his curves.

A similar statement is made in the report of the committee of the Franklin Institute on the "Strength of Materials employed in the Construction of Steam Boilers," viz., that the tests between the temperatures of zero and 520 degrees Fahrenheit were not sufficiently numerous to determine exactly the shape of the portion of the curve corresponding to those temperatures. These experiments will be discussed more particularly further on; at present we will only remark that in these experiments the increase of strength at temperatures between 400 and 580 degrees Fahrenheit over the strength at ordinary atmospheric temperatures was so marked that it was used as the basis for a mathematical expression of the law according to which the decrease of strength at higher temperatures apparently took place.

Styffe's experiments were made with round and square bars of iron and steel, having a diameter of about one-half inch. The steel bars contained from 0·33 to 1·14 per cent. carbon. The test pieces were immersed in a bath, the tempecature of which was measured by thermometers. Tests were made at low temperatures as well as at high temperatures, viz., ranging from 10 degrees Fahrenheit *below* zero to 412 degrees Fahrenheit *above* zero. The increase of strength with increase of temperature varied greatly not only for different materials, but in different bars of the same brand and of the same chemical composition as far as ascertained. As each individual bar furnished only a few test pieces, we cannot determine the *rate* at which the increase of strength took place in these experiments. In plotting the relative strength at high temperatures the mean strength of all the bars of the same brand at ordinary atmospheric temperature has been taken as unity.

Styffe sums up the results of these experiments in the following words, viz.: "From these experiments on tension at widely different temperatures we have thus found:

"1. That the absolute strength of iron and steel is not diminished by cold, but that, even at the lowest temperature which ever occurs in Sweden, it is at least as great as at the ordinary temperature (about 60° Fahr.)

"2. That at temperatures between 212° and 392° Fahr. the abso-

lute strength of steel is nearly the same as at the ordinary temperature; but in soft iron it is always greater."

Fairbairn's experiments were made with 14 specimens, all cut from the same bar of English rivet iron, $\frac{7}{8}$ inch in diameter. The results exhibit considerable regularity. The specimens were immersed during the tests in a bath, the temperature of which was measured by a thermometer. The mean values of the results for certain ranges of temperature have been plotted in the plate. At ordinary atmospheric temperature the strength of the test pieces was 63,000 pounds per square inch.

One specimen, cut from the same bar, was raised to a "red heat visible in daylight," but the temperature was not determined more accurately. In plotting the result of this experiment the temperature has been assumed to have been about 1000 degrees Fahrenheit.

Fairbairn has made another set of experiments with "Staffordshire" boiler-plates at temperatures ranging between zero and 395 degrees Fahrenheit. But owing to the want of uniformity in the quality of these plates, no law of increase or decrease of strength due to changes in temperature can be deduced from these experiments, and Fairbairn himself concluded that between 0 and 400 degrees Fahrenheit the strength of Staffordshire boiler-plates might be regarded as uniform. In one case a plate was heated until it became "perceptibly luminous in the shade" (or to "a scarcely perceptible red heat," as it is called in another place), and in another case a plate was raised to a "dull red heat just perceptible in daylight." No attempt was made to determine the temperatures of these plates more accurately. The results of these two experiments have been plotted, but the values have to be regarded simply as rough approximations.

The experiments by the British Admiralty, the results of which are plotted in the plate, were made in 1877 at Portsmouth dockyard. A somewhat meagre account of them is given in the "Engineer" of October 5, 1877. The strength of the test pieces at ordinary atmospheric temperature was as follows, viz.:

Remanufactured wrought iron rods, 0·74 inch diameter = 58,400 pounds per square inch.

Yorkshire wrought iron rods, 0·70 inch diameter = 59,200 pounds per square inch.

Landore steel strips, 0·74 inch by 0·49 inch = 76,900 pounds per square inch.

That the strength of wrought iron plates and bars increases with an increase of temperature up to about 500 degrees Fahrenheit, and that the amount and rate of this increase of strength with temperature varies greatly, not only for different brands, but for different specimens of the same brand, is a well-established fact; but it must be confessed that a satisfactory explanation of this peculiar behavior of wrought iron cannot be derived from the above-described experiments.

It is well known that the resistance of iron to strains at different temperatures varies according to its chemical composition; it is only necessary to allude to the phenomena of *cold-shortness* and *red-shortness* produced by the presence of certain quantities of phosphorus, sulphur and other ingredients. When the quantity of carbon combined with iron exceeds a certain limit, an increase of temperature is not accompanied by an increase of strength. In the British Admiralty's experiments cast iron was stronger at ordinary atmospheric temperature than at higher temperatures. In Styffe's experiments, different kinds of steel containing from 0·33 to 1·14 per cent. of carbon exhibited, on the whole, no increase of strength at higher temperatures. When the amount of carbon in wrought iron is 0·21 per cent. or less, its influence on the increased tensile strength of the test pieces at higher temperatures is not apparent in these experiments; nor can a connection be traced between the amount of phosphorus present in the iron and the strength of the latter at higher temperatures. The amounts of these two substances in the specimens of wrought iron tested by Styffe were as follows, viz.:

	Carbon.	Phosphorus.
Lowmoor iron,	0·21 per cent.	0·068 per cent.
Motala iron,	0·20 per cent.	0·020 per cent.
Middlesborough iron,	0·07 per cent.	0·25 per cent.
Aryd iron,	0·07 —0·18 per cent.	0·25 per cent.

It seems probable that the property of iron under consideration is dependent directly on the internal structure of the metal, and on its chemical composition mainly in so far as it determines to a certain extent the aggregation of the particles of the metal, viz., whether it be fibrous, granular or crystalline. To understand clearly the structural changes undergone by iron in the process of manufacture we must know the amount of reduction it has received, or the proportion of cross-section of the pile or bloom to that of the finished article, besides the manner in which, and the temperature at which, the reduction has

taken place. That these conditions influence greatly the ultimate strength as well as the elastic limit and the ductility of iron is well known; likewise, that by the process of rolling various permanent strains of greater or less magnitude are produced in every bar and plate, in consequence of the different amount of work expended upon, and compression experienced by, different parts of the bar or plate at the same cross-section. In steel bars and plates these internal strains are often so great that only a small additional strain is needed to produce fracture; but these internal strains may generally be relieved by annealing, as the peculiar granular or crystalline structure of steel seems to favor a re-arrangement of its particles at moderately high temperatures. In the case of wrought iron annealing by heating and gradual cooling produces a less marked effect in this respect. Considering wrought iron plates and bars as consisting of an aggregation of fibres, we may suppose that these fibres experience at ordinary temperatures an unequal amount of internal tension, which is relieved to a certain extent as the body expands and the fibres change their relative position in consequence of a rise of temperature; but that, with a fall of temperature, the fibres return to their original position and condition; and that thus only during the continuance of higher temperatures the internal strains are diminished and external strains are evenly distributed among the several fibres. On this hypothesis an increase of temperature will produce an increase of strength till the weakening effect of the repulsive action of heat on the molecules of the body counterbalances the effect of a diminution of internal strains and a more equal distribution of the strains produced by the load among the fibres. On the whole, the influence of temperature will probably be the more marked, the more fibrous the structure of the iron and the more work it has received at low temperatures. Round and square bars will probably show the influence of temperature more than flat bars and plates under otherwise equal conditions.

While these conclusions are consistent with the results of the above-described experiments, they cannot be considered as definitely proven; and they are given here mainly to indicate in what direction further inquiries into this subject may be made to advantage. The fact that Kollmann, contrary to all other experience, found no increase of strength at higher temperatures does not necessarily indicate that his observations were incorrect or his methods insufficient; but it may have been due to the character of the metal tested by him. It is

stated that the iron from which he took his test specimens entered the rolls at a temperature of about 2100 degrees Fahrenheit, and left them in the finished state at a temperature of about 1700 degrees Fahrenheit.

In this connection it is also interesting to compare the results obtained by Wertheim, Pisati and others with iron and steel wire. We select the following example from Pisati's experiments: The wire had been annealed at a dark-red heat; the strength decreased as the temperature rose from 57 to 122 degrees Fahrenheit; then it rose up to 192 degrees Fahrenheit, and fell again rapidly to 248 degrees, after which it remained constant up to 392 degrees, and after falling slowly to 455 degrees it rose again suddenly and then fell slowly; but at 572 degrees Fahrenheit the strength was still greater than at 57 degrees Fahrenheit. (See "Poggendorf's Annalen," vol. i, 1877.)

We have only a single series of experiments to compare with Kollmann's tests at temperatures exceeding 500 degrees Fahrenheit, viz., those made by a committee of the Franklin Institute in 1832–36. For the details of these experiments we have to refer the reader to the report of the committee. The manner of preparing the test pieces, and the method of computing mean values from the results of these early experiments are, no doubt, open to criticism; but the curve representing the rate of decrease of strength of wrought iron at high temperatures, given in the original report, is so strikingly similar to the curves representing the results of Kollmann's experiments, that we are compelled to believe that the general law of the decrease of strength of iron at high temperatures is correctly represented by these curves. The original curve representing the results of the Franklin Institute experiments gives, however, a much smaller decrease of strength at high temperatures than that found by Kollmann's experiments; but a closer investigation shows that this discrepancy did not exist between the *actual* results of these two sets of experiments, as will be seen from the following, viz.:

In the report of the Franklin Institute experiments the strength at high temperatures is not compared directly with the strength at ordinary atmospheric temperature, but with a standard of *maximum tenacity*, which is assumed to be 15·17 per cent. greater than the tenacity of the specimens at ordinary atmospheric temperature, for this was found to be the mean of the highest increase of tenacity corresponding to an increase of temperature above atmospheric temperature, deduced from a large number of tests.

In laying down the corrected curve of the Franklin Institute experiments in this paper, the tenacity of iron at ordinary atmospheric temperature has been taken as unity.

Further, in the Franklin Institute experiments the test pieces were kept immersed in a hot bath, and when the temperature of the bath was too high to be accurately measured with a mercurial thermometer, it was determined in the following manner, viz.: A piece of iron of a known weight, which had been kept immersed in this hot bath, was plunged into a vessel containing a known weight of water of a known temperature; the weight of water vaporized in this manner was used for computing the temperature of the piece of iron corresponding to the temperature of the bath and of the test piece; but in making this computation the latent heat of steam of atmospheric pressure was assumed to be 1037 degrees Fahrenheit, instead of 966 degrees Fahrenheit, as determined subsequently by Regnault; and the specific heat of iron was taken to be 0·11336 and uniform for all temperatures, while according to the formula by which Kollmann's temperatures were determined the specific heat of wrought iron is much greater at high temperatures. In consequence of these differences the temperatures given in the report of the committee of the Franklin Institute are much greater than those calculated by Kollmann's formula. In laying down the curve in the present paper the temperatures have been corrected by using the constants used in Kollmann's computations.

For temperatures above 500 degrêes Fahrenheit the corrected curve of the Franklin Institute experiments agrees to a remarkable degree with Kollmann's curves, especially with the curve for fine-grained iron, which it intersects twice and to which it is tangent at the point corresponding to a temperature of 1000 degrees Fahrenheit. A noteworthy fact is that at the point corresponding to this same temperature both curves representing Kollmann's tests of wrought iron as well as the Franklin Institute curve change suddenly in direction and in a similar manner. The Bessemer steel curve exhibits a much more rapid decrease of strength than the wrought iron curves, and its change of direction takes place at a temperature of about 775 degrees Fahrenheit.

It is a very interesting fact that the range of temperatures (viz., from 500 to 1000 degrees Fahrenheit) for which these curves exhibit such a remarkable rate of decrease of tenacity, coincides almost exactly with that at which wrought iron and mild steel possess a

remarkable weakness with respect to percussive forces, as was first announced by Daniel Adamson in a paper read before the Iron and Steel Institute, from which we quote the following:

"*Nearly all ordinary bar and boiler iron and mild steels will endure considerable percussive force when cold, and up to 450 degrees Fahrenheit, after which, as the heat is increased, probably to near 700 degrees Fahrenheit, they are all more or less treacherous and liable to break up suddenly by percussive action.*" (See "Journal of the Iron and Steel Institute," No. 2, 1878, p. 396.)

At higher temperatures these metals are free from this singular weakness. Adamson's observations have been verified by later experiments made by the British Admiralty.

Unfortunately we possess but very scant information regarding the effect of high temperature on the ductility and the limit of elasticity of wrought iron and steel.

Styffe found from his experiments "that neither in steel nor in iron is the *extensibility* less in severe cold than at the ordinary temperature; but that from 266 to 320 degrees Fahrenheit it is generally diminished, not to any great extent, indeed, in steel, but considerably in iron," and "that the *limit of elasticity* in both steel and iron lies higher in severe cold; but that at about 284 degrees Fahrenheit it is lower, at least in iron, than at the ordinary temperature."

It must be remembered that in Styffe's experiments the steels contained from 0·33 to 1·14 per cent. of carbon.

The following table exhibits the ductility of the wrought iron and steel specimens tested by the British Admiralty at Portsmouth dockyard in 1877. The second, third and fourth columns contain the elongation of the specimens after rupture, expressed in per cent. of the original length of 10 inches.

Temperature in degrees Fahr.	Wrought Iron. Remanufactured Rods.	Wrought Iron. Yorkshire Rods.	Landore Steel Strips.
Ordinary temperature.	22·	25·	26·
100	18·75	24·25	
150	15·		
200	15·	17·25	22·5
250	15·		
300	15·5	7·5	11·25
350	12·5		
400	12·	15·	10·25
450	15·		
500	20·	13·75	10·

The manner in which Kollmann's experiments were made did not permit an accurate determination of the ductility of iron at high temperatures. The general conclusion drawn by him from the results of his experiments, which are, however, very irregular, is that the elongation increases with a rise of temperature up to 850 degrees Fahrenheit for wrought iron, and even higher for Bessemer steel. He promises to give further investigations of this matter at a future day.

Kollmann's method of heating his test pieces in a furnace and allowing them to cool off during the process of testing was obviously defective, and the results obtained by him can only be regarded as rough approximations. Although the tests did not last more than half a minute, the specimens cooled off fully 100 degrees Fahrenheit at the higher temperatures; and even a difference of 50 degrees Fahrenheit would affect materially the tenacity of iron between the temperatures of 500 and 1000 degrees Fahrenheit. The great rapidity with which the tests had to be carried on was another source of inaccuracies. In order to observe carefully and measure accurately the effect of heat upon the tenacity, ductility and elasticity of iron, the specimens must be maintained at a uniform temperature by keeping them immersed in a bath. This can be readily done as long as the temperature does not exceed 1000 degrees Fahrenheit. Beyond that temperature Kollmann's method of heating the test pieces is probably the least troublesome and sufficiently accurate.

The importance of making further investigations of the subject discussed in this paper need not be dwelt upon. We wish, however, to call the special attention of manufacturers of rolled iron and steel plates and bars to the immediate advantages which they may expect to reap from similar investigations. Carefully conducted and intelligently analyzed experiments relating to the behavior of iron at high temperatures will enable them to estimate correctly the relative value of various methods of treating the metal in the process of rolling, and may also throw some light on the obscure causes producing those internal strains which often render rolled iron of very uncertain value with regard to strength, ductility and elasticity.

References: "Ueber die Festigkeit des erhitzten Eisens," von Dr. Julius Kollmann; Berlin, 1880. "Useful information for Engineers," by W. Fairbairn; second series. "Iron and Steel," by Knut Styffe; translated from the Swedish by C. P. Sandberg; London, 1869. "Report of the Committee of the Franklin Institute on the Explosions of Steam Boilers," Part II; Philadelphia, 1837. "Tenacity of Metals at various Temperatures;" *Engineer*, October 5, 1877.

NOTE RELATING TO THE PROPER METHOD OF EXPANSION OF STEAM AND REGULATION OF THE ENGINE.

By Prof. R. H. Thurston.

Read before the American Society of Mechanical Engineers, Altoona Meeting, August, 1881.

It has long been well known, to every engineer experienced in the construction and management of the steam engine, that when working under known conditions and at a given pressure of steam, there is a certain ratio of expansion which gives highest efficiency, *i. e.*, least expenditure of fuel in proportion to work done. It has also been long known that the most economical ratio of expansion, all things considered, when studied from the commercial standpoint, is to be determined not simply by studying those conditions which affect the efficiency of the engine, but by consideration of all the elements of cost of steam power, including first cost, interest on capital expended, wear and tear and running expenses for fuel supply and management.

In the design of an engine it thus becomes necessary, if the designer would consult the best interests of the purchaser of the engine, first to determine as best he can what is likely to be the cost of each series of engines of the style proposed and of graded sizes; next must be determined the cost of power in weight of steam used in each of these engines at various pressures and ratios of expansion; finally, by comparison, he must select that engine which at the least ratio of expansion, all things considered, will give the required power.

When the steam pressure and the cost of power in steam consumption are known, it becomes possible to determine the best point of cut-off. But no engineer has yet been able to say with certainty what, in any given case, will be the ratio of expansion giving highest efficiency, or what, at any given rate, will be the probable expenditure of steam or fuel. The writer has endeavored, in an earlier paper,* to show what are the causes of this uncertainty, what determines the most efficient adjustment of expansion, and what, on the whole, have, in his judgment, proved to be the ratios of expansion giving maximum effi-

* "On the Ratio of Expansion at Maximum Efficiency," Trans Am. Soc. Mech. Engrs., 1881; Journal Frank. Inst., May, 1881.

ciency, and, finally, what efficiency may probably be anticipated in cost of steam and of fuel in good engines, adjusted for maximum efficiency.

When these points are settled the engineer may, by a proper use of the factors, ascertained as above indicated, determine what is the best ratio of expansion to adopt, or rather what is the best size of engine for the case in hand, and what the best type of valve gear and regulating mechanism. The size and kind of engines is therefore determinable from a knowledge of conditions, which are partly physical and incidental to construction, and partly commercial.

The solution of this second and broader problem may be effected either by a tentative process of trial and repeated estimation, or by an approximate, and for some cases—*e. g.*, where cylinder condensation is reduced to a minimum, as in efficiently jacketed or in fast-moving engines, or at low rates of expansion—nearly exact method, first indicated, so far as the writer is aware, by Professor Rankine,* who applies to the case one of those beautiful graphical constructions in the devising of which he was so ingenious and successful.

This method has been studied and has been applied to representative examples in recent practice in steam engineering by Messrs. Wolff & Denton,† who have shown that the commercially profitable grades of expansion are ordinarily restricted to a very narrow range, and always somewhat less than the ratio for maximum efficiency.

Having, then, fixed upon the size of engine and ratio of expansion, it is evident that this ratio of expansion should generally be kept invariable, so long as the steam pressure remains unchanged.

The usual changeable condition with a given engine is the demand for power, and to meet this variation it becomes necessary to adopt some method of regulation. The simplest forms of regulating apparatus usually consist of a "fly-ball" governor set to operate a "throttle-valve" or other kind of regulating valve; the most usual method of regulation with the better class of engines is that adopted by Corliss—the attachment of the governor to the expansion gear in such a manner as to cause a variation of the ratio of expansion, adjusting the point of cut-off to the demand for power. This latter is the most sensitive regulating mechanism yet devised, and where the variation

* Phil. Magazine; Trans. R. S. E.; Theory and Practice of Shipbuilding; Miscellaneous Papers.

† Trans. Am. Soc. Mech. Engr's, 1881; The American Engineer, 1881.

is small is very effective even at low speeds. The writer has counted the revolutions of a Corliss engine making about sixty revolutions per minute, with steam at 90 lbs. by gauge (7 atmospheres), and found a variation of but two revolutions per minute when the whole load was thrown off or on, the minimum being about 35 horse-power (driving shafting) and the maximum about 150.

Since, however, the proper ratio of expansion for the engine when once installed is determined mainly by the steam pressure, and since any variation from that point is usually productive of reduction of efficiency, it would seem that the ratio should be fixed at the best proportion for the steam pressure adopted and never changed. This being the case, the question arises, how shall regulation be effected? The adjustment of a throttle-valve by the governor is inadmissible, as it involves variation of the steam pressure in the steam chest and consequently reduced efficiency; the steam and expansion lines must be permanently fixed for all loads.

It becomes at once evident that any allowable system of regulation must now affect the back pressure or the cushion line. To throttle the exhaust by the action of the governor would undoubtedly give a means of regulation, but a costly one; since any increase of back pressure during the exhaust would involve serious increase in the amount of rejected heat and of waste of power.

It then becomes evident that the only admissible plan is the variation of the net power of the engine by an alteration of the compression line. This is done where one very well known and generally used valve gear is adopted—the Stephenson Link Motion. When the link is down the ratio of expansion is determined by the lap and lead, and is usually not higher than $\frac{4}{3}$; as the link is raised this ratio is increased, and with this change of the steam line occurs a simultaneous alteration of the point of release and of closure of the exhaust passages, resulting in increased compression. This double effect gives the indicator diagram a peculiar modification of form, familiar to engineers who have taken cards from the locomotive or the usual type of marine engine. The smoothness of working of such engines when running with high steam and a raised link has probably been observed by all experienced engineers, and it may not have escaped notice that under such conditions the expenditure of steam is often so low as to indicate some source of economy other than simple change in the ratio of expansion.

The writer, at least, when in charge of naval steam machinery during the war of 1861–5, was led to suspect a gain from this distribution of steam, which could only be attributed to what was considered excessive compression.

His attention has recently been called to this matter again by the interesting results of a series of experiments made upon a large engine fitted with variable expansion gear. The valve motion is so arranged as to permit adjustment of compression without change of either steam, expansion or exhaust lines. The results will be reported in a later paper. It is only necessary here to state that a decided gain is found to follow the adjustment of the compression to a far higher ratio than is indicated as best by the simple geometrical conditions usually studied and generally taken as those determining the proper ratio of compression. This beneficial effect of a high ratio of compression has been attributed by the writer to the action of the compressed fluid in heating the passages and the cylinder head and piston, thus checking to a very great extent that initial cylinder condensation which is the greatest source of avoidable waste in nearly all engines.

It may be asserted that the best compression, where no such transfer of heat occurs, is not far from that which makes the ratios of expansion and compression equal, and the engineer will usually set the exhaust valve to close at the point corresponding to maximum expansion. For the reasons just given, however, and as shown by direct experiment, maximum efficiency is obtained with higher ratios of compression, and what would have been considered excessive cushioning gives less loss than equal variation from the point of cut-off giving maximum efficiency. As compression is increased, the area of the indicator diagram decreases and the work developed in the engine becomes less.

It would seem, then, that we have here an admissible method of regulation and one which should be, on the whole, that best fitted to give high efficiency, since any excess of work of compression results simply in the transfer of heat back to the steam side. The steam engine should, therefore, be worked with a fixed cut-off,* so attaching the governor as to determine the point of closing of the exhaust valve—in other words, making the "cut-off" operate on the exhaust

*The writer has devised methods of automatic readjustment of the ratio of expansion when variations occur in the steam pressure, which methods would in the case here taken replace the usual adjustment by the governor.

side, the ratio of compression being determined by the governor—instead of attaching the cut-off mechanism to the steam valves. Properly constructed relief valves will prevent all danger from the influx of water with the steam, an accident which, however, should never occur where provision is made for securing dry steam. With exhaust ports beneath the cylinder, drainage is rarely imperfect.

In slowly moving pumping engines it has sometimes been found beneficial to extend compression until boiler pressure is exceeded, and the writer has indicator diagrams taken from such engines in which the compression line crosses the steam line before the end of the return stroke has been reached. He has, as probably has every engineer who has been accustomed to handle locomotive or marine engines, often set the link motion so as to give such high ratios of expansion and compression as to reduce the card to a comparatively narrow band, without perceiving the slightest evidence of objectionable loss of smoothness of working and with decidedly improved efficiency. It seems to the writer doubtful whether, in practice, objectionable or "excessive" compression ever occurs in such cases, and the advantages of this method of regulation and of securing a lessened variability in the ratio of expansion would appear to be decided and to be obtainable without meeting with serious difficulties.

The plan is probably not entirely a novel one and may have suggested itself to many engineers independently; but no attempt has previously, so far as the writer is aware, been made to determine its advantages in actual work. The writer indicated this as the proper method of adjusting expansion some years ago,* and has since had it presented to him by other engineers with whom the thought was also original.

Where the plan here suggested cannot be adopted conveniently, maximum economy of steam should be obtained by an expansion gear in which, as in locomotive valve gear, increased expansion is accompanied by increased compression, but without that serious throttling along the steam line which usually characterizes the distribution by the link-motion.

The best among existing forms of valve gear should be, if judged from the standpoint here taken, that which—combining a variable expansion with a variable compression—is also capable of prompt and exact adjustment by a sensitive and efficient governor.

*History of the Steam Engine. D. Appleton & Co. (International Series), N. Y., 1878, p. 473, foot note.

The economy to be expected from the suggested change in methods of regulation of the steam engine will evidently be dependent upon the manner of operation of the engine. Where engines have a nearly invariable load, and when they are well adjusted to their work, the advantage would probably be found inappreciable; but in cases in which the engine is much too large for its work or when the demand for power is very irregular, as in rolling mills or in rough weather at sea, and where cylinder condensation occurs to a great extent, the increased efficiency may be found to be very considerable. The gain by decreased internal condensation will, perhaps, often be found to be an item of no small importance.

Hoboken, N. J., June, 1881.

ON THE LAST EXPERIMENT (19TH MARCH, 1881) WITH THE PERKINS MACHINERY OF THE STEAM YACHT "ANTHRACITE."

By Chief-Engineer ISHERWOOD, U. S. Navy.

In the January, February and March numbers of the present year of this journal will be found a description of the Perkins system of steam machinery in the steam yacht *Anthracite*, together with an account of the two experiments which had been made with it to determine its economic efficiency. The first experiment was made in England, with the vessel in free route, by Mr. F. J. Bramwell, an engineering expert employed for that purpose by the "Perkins Engine Company," and the second was made by a Board of Chief-Engineers of the United States Navy with the vessel secured to the dock of the New York Navy Yard.

There being a wide discrepancy between the results of these two experiments, the "Perkins Engine Company," on the return of the *Anthracite* to England, caused Mr. Bramwell to repeat his original experiment; and it is the object of this paper to give the data and results of the repetition—which are substantially the same as those of his original experiment—as a further contribution to a knowledge of what economy may be expected from the employment in a steam engine of steam of exceptionally high pressure used with a correspondingly great measure of expansion.

The three experiments above referred to, being all that ever were made with steam of the excessive pressure employed and so enormously expanded, have a peculiar value on account of their fewness and of their greatly exaggerated conditions of pressure and expansion, nothing remotely approaching either being found in ordinary practice.

Before giving the data and results of the repeated experiment, it is necessary to impress the reader with the fact that the three experiments above referred to, being all made under substantially the same conditions of high pressure and great expansion, have nothing with which they can be properly compared.

The contention of the "Perkins Engine Company" is that the higher the pressure of steam and the more it is expanded, the greater will be the economy. To show this effect, one or many experiments on the same machinery, if all be made under the same conditions, are insufficient. A series is necessary, with varying conditions, beginning with the highest pressure and greatest expansion and gradually reducing both, ending with the least pressure and expansion of ordinary practice, ascertaining experimentally the results with each reduction; thus proving or disproving, as the event may be, whether or not the position of the "Perkins Engine Company" is well- or ill-founded. That Company, however, has not made such experiments; and the two conducted by Mr. Bramwell are incomplete in the non-ascertainment of the weight of feed-water consumed per hour. In the one conducted by the Board of United States Naval Engineers, however, this weight was determined.

As part of the economic effect of an engine, measured by the cost of the power it develops in pounds of coal consumed per hour, is due to the quality of the coal, part to the type and proportions of the boiler, and the remainder to the type and proportions of the engine—including therewith the pressure, quality and expansion of the steam used, it is necessary to separate these factors and ascertain what portion of the whole effect is due to each. Now, in the experiments made by Mr. Bramwell, the finest description of hand-picked Nixon's navigation coal was burned, a coal of exceptional quality for generating steam and of proportionally increased cost; and, manifestly, whatever may be the difference in weight of water vaporized in a boiler by unity of weight of this coal and by others of very considerably inferior quality, should not be attributed to the machinery; so

that Mr. Bramwell's experiments fail at the outset in furnishing data with the Perkins engine for any proper comparison with engines using steam generated by inferior qualities of coal.

Mr. Bramwell's last experiment shows that the steam furnished by the Perkins boiler was not superheated. Now, as that boiler contains 14·7227 square feet of water-heating surface, measured on the interior surface of the tubes, or 19·6033 square feet measured on their exterior surface, and 15·6179 square feet of steam superheating surface, measured on the interior surface of the tubes, or 21·2178 square feet measured on their exterior surface—all per square foot of grate surface—there follows that, practically, this boiler had (14·7227+15·6179=) 30·3406 square feet of water-heating surface, measured on the interior surface of the tubes, or (19·6033+21·2178=) 40·8211 square feet, measured on the exterior surface of the tubes; because, if water had not been on the whole of this surface, the steam must have shown some superheating; and, had the water been on only the normal water-heating surface, the steam would certainly have been excessively superheated with so great a proportion of steam superheating surface to grate surface. Thus the Perkins boiler, though furnishing only saturated steam, had an exceptionally large proportion of water-heating to grate surface; in virtue of which, the pound of coal would vaporize in it a larger quantity of water than in a boiler in which the ratio of the water-heating to the grate surface was the much less proportion employed in ordinary practice. In this connection, too, must be remarked that the rate of combustion in the Perkins boiler, for a full power trial, was less than half the rate of ordinary practice with the coal used; so that less than half the quantity of heat per hour was thrown upon about one-third more heat-absorbing surface, producing, of course, the greater economic vaporization due to these exceptionally favorable conditions.

Before Mr. Bramwell's last experiment was made, the pitch of the *Anthracite's* screw was largely increased, so that with a given weight of steam passed through the engine per hour, the steam pressures in the cylinders would all be higher and the piston speed lower than in his first experiment.

Before the system according to which the *Anthracite's* machinery was designed and is worked can be accepted as experimentally proven to be economically superior to any other in use, the weight of feed-water consumed per hour, as well as the power developed, must be

ascertained for a series of pressures and expansions, the results of all which shall be found to converge in the same direction, showing an increased economy for each increase in the pressure and in the expansion. Until this is done, however, the experiments that have been made are of great interest; the data and results of the last being as follows, excepting that the writer has supplied from other sources an approximate estimate of the weight of feed-water consumed per hour and the deductions drawn therefrom. None of the quantities in the following table are to be found in Mr. Bramwell's report, but the writer has obtained them from the data therein given, which includes a reproduction of all the indicator diagrams taken. In fact, the report itself contains but little more than the observed data of the experiment, leaving the reader to make his own calculations and deductions.

The writer has selected from the data of the report the ten hours of the performance during which the machinery was operated with the fire in full and steady action, supplying a uniform weight of steam of uniform pressure per hour. It is assumed that at the end of these ten hours the fire was in the same condition as regards cleanness and thickness, and that the quantity of water in the boiler and the pressure of the steam were the same as at the beginning, which they undoubtedly were as nearly as could be judged. A set of indicator diagrams were taken every half hour; a set comprising a diagram from the top of the 1st cylinder, a diagram from the bottom of the 2d cylinder and a diagram from the top and bottom of the 3d cylinder. The steam and other pressures and the temperatures were noted half hourly. The machinery worked well and smoothly, with great regularity and entire freedom from hot journals.

The calculations from the data have been made by the writer in exactly the same manner as for the two previous experiments.

Table Containing the Data and Results of the Experiment made in England, by Mr. F. J. Bramwell, on the Machinery of the Steam Yacht "Anthracite," to Determine its Economic Performance.

Total Quantities.

Date of the experiment (vessel in free route), . .	19th March, 1881.
Number of sets of indicator diagrams, taken half hourly,	20
Duration of the experiment, in hours and minutes, consecutively, ...	10

Total number of pounds consumed of Nixon's navigation steam coal, ...	1770·8680
Total number of pounds of refuse, in ash, etc., from the coal, ...	88·5434
Total number of pounds of combustible (gasifiable portion of the coal) consumed, ...	1682·3246
Per centum of the coal in refuse of ash, . .	5·
Total number of double strokes made by the pistons of the engine, ...	63650·

ENGINE.

Steam pressure in the boiler, in pounds per square inch above the atmosphere, ...	395·15
Steam pressure in the receiver, in pounds per square inch above the atmosphere, ...	18·90
Position of the throttle valve, . . .	Wide open.
Fraction completed of the stroke of the piston of the 1st cylinder when the steam was cut off, . .	0·6077
Fraction completed of the stroke of the piston of the 3d cylinder when the steam was cut off, . .	0·2643
Number of times the steam was expanded, .	22·9154
In none of the cylinders was the steam cushioned, nor was there either steam or exhaust lead, . .	....
Vacuum in the condenser, in inches of mercury .	28·75
Back pressure in the condenser, in pounds per square inch above zero, ...	0·575
Number of double strokes made per minute by the steam pistons, ...	106·0833

TEMPERATURES.

Probable temperature, in degrees Fahrenheit, of the feed water, ...	122·0
Temperature, in degrees Fahrenheit, of the boiler steam, considered as saturated, ...	438·0
Temperature, in degrees Fahrenheit, of the steam in the valve chest of the 1st cylinder, . . .	422·5
Temperature, in degrees Fahrenheit, of the steam in the 1st cylinder at the commencement of the stroke of the piston, considered as saturated, . . .	416·3
Temperature, in degrees Fahrenheit, of the air in the air space between the side of the 1st cylinder and its lagging, ...	314·6
Temperature, in degrees Fahrenheit, of the water of condensation from the steam jacket of the 1st cylinder, .	418·5
Temperature, in degrees Fahrenheit, of the water of condensation from the steam-jacket of the 2d cylinder, .	409·8
Temperature, in degrees Fahrenheit, of the water of condensation from the steam-jacket of the 3d cylinder, .	404·0

Rate of Combustion.

Pounds of coal consumed per hour,	177·0868
Pounds of combustible consumed per hour,	168·2325
Pounds of coal consumed per hour per square foot of grate,	11·5624
Pounds of combustible consumed per hour per square foot of grate,	10·9842
Pounds of coal consumed per hour per square foot of outer heating surface,	0·2852
Pounds of coal consumed per hour per square foot of inner heating surface,	0·5811
Pounds of combustible consumed per hour per square foot of outer heating surface,	0·2670
Pounds of combustible consumed per hour per square foot of inner heating surface,	0·5620

Steam Pressures in First Cylinder, Per Indicator.

Pressure on piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	317·60
Pressure on piston of 1st cylinder at the point of cutting off the steam, in pounds per square inch above zero,	261·44
Pressure on piston of 1st cylinder at end of its stroke, in pounds per square inch above zero,	175·94
Mean back pressure against piston of 1st cylinder during its stroke, in pounds per square inch above zero,	79·6000
Back pressure against piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	58·38
Indicated pressure on piston of 1st cylinder, in pounds per square inch,	176·0625
Net pressure on piston of 1st cylinder, in pounds per square inch,	174·0625
Total pressure on piston of 1st cylinder, in pounds per square inch,	255·6625

Steam Pressures in Second Cylinder, Per Indicator.

Pressure on piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero,	143·50
Pressure on piston of 2d cylinder at end of its stroke, in pounds per square inch above zero,	44·46
Mean back pressure against piston of 2d cylinder during its stroke, in pounds per square inch above zero,	28·6334
Back pressure against piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero,	36·50
Indicated pressure on piston of 2d cylinder, in pounds per square inch,	24·7840

Net pressure on piston of 2d cylinder, in pounds per square inch, ...	22·5840
Total pressure on piston of 2d cylinder, in pounds per square inch, for the difference between the areas of the pistons of the 1st and 2d cylinders, . . .	63·2174

Steam Pressures in Third Cylinder, Per Indicator.

Pressure on piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, .	33·58
Pressure on piston of 3d cylinder at the point of cutting off the steam, in pounds per square inch above zero, .	27·69
Pressure on piston of 3d cylinder at the end of its stroke, in pounds per square inch above zero, . .	10·03
Mean back pressure against piston of 3d cylinder during its stroke, in pounds per square inch above zero, .	3·0755
Back pressure against piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, ...	2·10
Indicated pressure on piston of 3d cylinder, in pounds per square inch, ...	16·3612
Net pressure on piston of 3d cylinder, in pounds per square inch, ...	14·3612
Total pressure on piston of 3d cylinder, in pounds per square inch, for the difference between the areas of the pistons of the 2d and 3d cylinders, . . .	19·4367

Horses-Power.

Indicated horses-power developed in the 1st cylinder, .	33·3736
Indicated horses-power developed in the 2d cylinder, .	18·8080
Indicated horses-power developed in the 3d cylinder, .	53·3489
Aggregate indicated horses-power developed in all three cylinders, ...	105·5305
Net horses-power developed in the 1st cylinder, .	32·9945
Net horses-power developed in the 2d cylinder, .	17·2777
Net horses-power developed in the 3d cylinder, .	46·8301
Aggregate net horses-power developed in all three cylinders, ...	97·1023
Total horses-power developed in the 1st cylinder, .	48·4821
Total horses-power developed in the 2d cylinder, .	36·3558
Total horses-power developed in the 3d cylinder, .	33·6362
Aggregate total horses-power developed in all three cylinders, ...	118·4741
Total horses-power developed by the expanded steam alone in the 1st cylinder, ...	17·5092
Total horses-power developed by the expanded steam in the 2d cylinder, ...	36·3558
Total horses-power developed by the expanded steam in the 3d cylinder, ...	33·6362

Weight of Steam Accounted for by the Indicator.

Pounds of steam present per hour in the 1st cylinder at the point of cutting off the steam, calculated from the pressure there, ...	1043·9127
Pounds of steam present per hour in the 1st cylinder at the end of the stroke of its piston, calculated from the pressure there, ...	1125·2924
Pounds of steam condensed per hour in the 1st cylinder to furnish the heat transmuted into the total horses-power developed in that cylinder by the expanded steam alone, ...	42·9840
Sum of the two immediately preceding quantities, .	1168·2764
Pounds of steam present per hour in the 2d cylinder at the end of the stroke of its piston, calculated from the pressure there, ...	1192·7472
Pounds of steam condensed per hour in the 1st and 2d cylinders to furnish the heat transmuted into the total horses-powers developed in those cylinders by the expanded steam alone, ...	143·4642
Sum of the two immediately preceding quantities, .	1336·2114
Pounds of steam present per hour in the 3d cylinder at the end of the stroke of its piston, calculated from the pressure there, ...	1156·5362
Pounds of steam condensed per hour in the 1st, 2d and 3d cylinders to furnish the heat transmuted into the total horses-power developed in those cylinders by the expanded steam alone, ...	231·7806
Sum of the two immediately preceding quantities, .	1388·3168

Weight of Water Vaporized in the Boiler from the Feed Temperature.

Pounds of steam evaporated per hour in the boiler, .	1830·0000

Note.—In Mr. Bramwell's previous experiment of the 22d May, 1880, the pound of precisely the same coal vaporized, from the temperature of 122 degrees Fahrenheit and under the pressure of 371·69 pounds per square inch above zero, 10·118407 pounds of water; consequently, in the present experiment, it will vaporize from the same temperature, but under the pressure of 109·84 pounds per square inch above zero, 10·381482 pounds of water. Practically, this latter vaporization will be a little less, owing to the greater rapidity of the combustion and to the higher temperature of the iron heating surfaces of the boiler, and may be taken at 10½ pounds of water per pound of coal, from which the weight of steam evaporated per hour in the boiler, as given above, was calculated.

Difference between the Weight of Water Vaporized in the Boiler and the Weight of Steam Accounted for by the Indicator.

Difference, in pounds per hour, between the weight of water vaporized (1830 pounds) in the boiler and the weight of steam

accounted for by the indicator in the 1st cylinder at the point of cutting off the steam,	786·0873
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the point of cutting off the steam,	42·96
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 1st cylinder at the end of the stroke of its piston,	661·7236
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the end of the stroke of its piston,	36·16
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston,	493·7886
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston,	26·98
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston,	441·6832
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston,	24·14

Economic Results.

Pounds of coal consumed per hour per indicated horse-power,	1·6781
Pounds of coal consumed per hour per net horse-power,	1·8237
Pounds of coal consumed per hour per total horse-power,	1·4947
Pounds of combustible consumed per hour per indicated horse-power,	1·5951
Pounds of combustible consumed per hour per net horse-power,	1·7325
Pounds of combustible consumed per hour per total horse-power,	1·4200
Pounds of feed-water consumed per hour per indicated horse-power,	17·3410
Pounds of feed-water consumed per hour per net horse-power,	18·8461
Pounds of feed-water consumed per hour per total horse-power,	15·4464
Fahrenheit units of heat consumed per hour per indicated horse-power,	19515·3167
Fahrenheit units of heat consumed per hour per net horse-power,	21209·1901

Fahrenheit units of heat consumed per hour per total horse-power, ... 17584·2182

Per centum of Total Pressure on Pistons Utilized as Indicated and as Net Pressures.

Mean indicated pressure on the piston of the 3d cylinder, equivalent to the sum of the indicated pressure on that piston and of the indicated pressures on the pistons of the 2d and 1st cylinders, reduced respectively in the ratios of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting, while the 3d cylinder is double acting, in pounds per square inch, ... 32·5943

Mean total pressure which applied to the piston of the 3d cylinder would produce the total horses-power developed by the engine, ... 36·5840

Per centum of the mean total pressure on the pistons of the three cylinders, utilized as indicated pressure, ... 89·08

Mean net pressure on the piston of the 3d cylinder, equivalent to the sum of the net pressure on that piston and of the net pressures on the pistons of the 2d and 1st cylinders, reduced respectively in the ratios of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting, while the 3d cylinder is double acting, in pounds per square inch, . . . 29·9787

Per centum of the mean total pressure on the pistons of the three cylinders utilized as net pressure, ... 81·96

Boiler Vaporization.

Number of pounds of water that would have been vaporized in the boiler per hour had the feed-water been supplied at the temperature of 100 degrees Fahrenheit and vaporized under the atmospheric pressure of 29·92 inches of mercury, . 1935·5022

Number of pounds of water that would have been vaporized in the boiler per hour had the feed-water been supplied at the temperature of 212 degrees Fahrenheit and vaporized under the atmospheric pressure of 29·92 inches of mercury, . 2162·0698

Pounds of water vaporized from 100 degrees Fahrenheit by one pound of coal, ... 10·7823

Pounds of water vaporized from 100 degrees Fahrenheit by one pound of combustible, ... 11·5504

Pounds of water vaporized from 212 degrees Fahrenheit by one pound of coal, ... 12·0465

Pounds of water vaporized from 212 degrees Fahrenheit by one pound of combustible, ... 12·6742

COMPARISON OF MR. BRAMWELL'S FIRST AND LAST EXPERIMENTS.

As the screw of the *Anthracite* had a greater pitch, and as more coal was consumed per hour, during the last experiment than during the first, the conditions of the two varied as regards the number of double strokes made by the pistons per minute, the pressure of the steam and the power developed. The steam, too, was less expanded during the last experiment and less throttled, but it was greatly throttled in both, even with the throttle valve wide open, as in the last experiment, owing to the exceedingly small diameter of the steam pipe comparably with the space displacement per minute of the piston of the 1st cylinder.

In the last experiment, as compared with the first, the steam pressures in boiler and cylinders were much higher, the back pressure in the condenser much lower, the number of double strokes made by the pistons per minute much less, and the weight of coal consumed per hour much greater. To what extent each of these variable conditions affected the final result of the cost of the power in pounds of coal consumed per hour cannot be determined; but the result of their combination, if the comparison be made for the cost of the total horse-power in pounds of coal consumed per hour, which is the proper engineering comparison under the conditions, shows that the greater pressures, less expansion and less piston speed (the last experiment), gave $\left(\frac{1{\cdot}4947-1{\cdot}4291\times100}{1{\cdot}4291}=\right)$ 4·5903 per centum less economic efficiency than the lesser pressures, greater expansion and greater piston speed (first experiment). But as this difference may easily be within the limits of error for such experiments, nothing can be fairly concluded except for equality of economic performance in the two cases. The greater economy with which, on the contrary, the indicated horse-power was obtained during the use of the greater pressures, less expansion and less piston speed, was due simply to the better vacuum in the condenser in that experiment, which might, except for accidental circumstances, have been equally good for both experiments. The lower the mean total pressure in the large cylinder, the more important becomes the back pressure in the condenser. The still greater economy with which the net horse-power was obtained during the last experiment was due to the same cause, increased by the fact that the pressure required to work the engine, *per se*, being the same in both cases, was a larger proportion of the

less total piston pressure in the first experiment than of the greater total piston pressure in the last experiment. This realization usefully of a larger proportion of the total pressure on a piston when that pressure is higher than when it is lower, owing to the facts that the back pressure and the pressure required to work the engine, *per se*, are constant, is an undoubted advantage for higher mean total pressures on the piston, which will always exist, let the effect of other causes be what they may.

The two experiments clearly show that the cylinder condensations, exclusive of the condensation due to the development of the power by the expanded steam alone, were larger during the last experiment than during the first, a result which might have been anticipated from the greater initial pressure in the 1st cylinder and less back pressure in the 3d cylinder, joined with the much less speed of piston, notwithstanding the less expansion of the steam in the last experiment than in the first.

From the following table, in which will be found the principal quantities that influence the comparison of the two experiments, a clear idea may be had of the extent of the variations in the experimental conditions and of the resulting effects.

ENGINE.

	Mr. [illegible] First Experiment.	Last Experiment.
Date of experiment, . .	22d May, 1880.	19th March, 1881.
Steam pressure in boiler in pounds per square inch above the atmosphere, .	357·00	395·15
Steam pressure in boiler in pounds per square inch above zero, . .	371·69	409·84
Position of throttle valve, .	Partly closed.	Wide open.
Number of times the steam was expanded, ...	26·8851	22·9154
Vacuum in the condenser in inches of mercury, ...	26·864	28·750
Number of double strokes made per minute by the pistons, . .	130·3881	106·0853

POWER.

Initial pressure on piston of 1st cylinder, in pounds per square inch above zero, ...	265·03	317·00
Back pressure against piston of 3d cylinder, in pounds per square inch above zero, . . .	4·245	2·100

	Mr. Bramwell's First Experiment.	Mr. Bramwell's Last Experiment.
Aggregate indicated horses-power developed by the engine, . .	80·7323	105·5305
Aggregate net horses-power developed by the engine, . . .	70·3703	97·1023
Aggregate total horses-power developed by the engine, . .	96·6795	118·4741

ECONOMIC RESULTS.

Pounds of coal consumed per hour per indicated horse-power, . .	1·7114	1·6781
Pounds of coal consumed per hour per net horse-power, . . .	1·9634	1·8237
Pounds of coal consumed per hour per total horse-power, . . .	1·4291	1·4947

EQUIVALENT PISTON PRESSURES.

Mean indicated pressure on the piston of the 3d cylinder, equivalent to the sum of the indicated pressure on that piston and of the indicated pressures on the pistons of the 2d and 1st cylinders, reduced respectively in the ratios of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting, while the 3d cylinder is double acting, in pounds per square inch, .	20·1464	32·3643
Mean net pressure on the piston of the 3d cylinder in pounds per square inch, equivalent to the sum of the net pressure on the piston and of the net pressures on the pistons of the 2d and 1st cylinders, reduced as described immediately above, .	17·5608	29·7787
Mean total pressure on the piston of the 3d cylinder in pounds per square inch, equivalent to the sum of the total pressure on that piston and of the total pressures on the pistons of the 2d and 1st cylinders, reduced as described immediately above, .	24·1231	36·3340

CYLINDER CONDENSATION.

Per centum of the weight of steam generated in the boiler, condensed in the 1st cylinder at the point of cutting off the steam, . . .	31·27	42·96

	M. [illegible]'s First Experiment	Last Experiment
Per centum of the weight of steam generated in the boiler, condensed in the 1st cylinder at the end of the stroke of its piston, exclusive of the condensation due to the development of power by the expanded steam alone, ...	34·99	36·16
Per centum of the weight of steam generated in the boiler, condensed in the 2d cylinder at the end of the stroke of its piston, exclusive of the condensation due to the development of power by the expanded steam alone, ...	21·53	26·98
Per centum of the weight of steam generated in the boiler, condensed in the 3d cylinder at the end of the stroke of its piston, exclusive of the condensation due to the development of the power by the expanded steam alone, ...	8·47	24·14
COMBUSTION.		
Pounds of coal consumed per hour per square foot of grate surface, .	9·0215	11·5624

Registering Apparatus for Marine Cables.—The most perfect apparatus which has hitherto appeared for giving graphic signals upon the great submarine lines of cable is Sir Wm. Thomson's syphon recorder. There would be a great advantage in a telegraph which would register the signals of the mirror galvanometer. Paul Samuel proposes to use two selenium elements upon the screen where the light from the galvanometer is reflected: one at the right, the other at the left. Whenever one of them is illuminated its increased conductibility will enable it to act as a relay upon an electro-magnet, for marking upon a paper band the points and dashes of the Morse alphabet. He recommends the use of paper dipped in iodide of potassium, and of a triangle which will be moved so that the point will be controlled by one of the selenium elements and the base by the other; in the former case a dot will be produced, in the latter a dash.—*Les Mondes.* C.

RADIO-DYNAMIC FACTS.

By PLINY EARLE CHASE, LL.D.
Professor of Philosophy in Haverford College.

Before proceeding to demonstrate the fundamental propositions of photodynamics it may be well to give a list of some of the most important FACTS which have been added to the repertory of science by investigating the laws of action and reaction in elastic media. The original announcements of discovery may be found in the "Proceedings of the American Philosophical Society," unless otherwise stated.

1. The importance of the equations of "nascent" or dissociative velocity, $v = \frac{g\,t}{2}$, and of nascent modulus, $h = \frac{gt^2}{4}$; t representing the time of cosmical, molecular, or atomic rotation, and g representing the acceleration of a central force. Dec. 18, 1863.

2. Sun's annual and Earth's daily disturbances of atmospheric elasticity furnish data for estimating the distance and relative masses of the two disturbing bodies. Dec. 18, 1863.

3. Modification of the daily distribution of solar heat by cyclical elasticity and barometric pressure. March 4, 1864.

4. Mechanical modification of electric and magnetic currents. April 1, 1864; Oct. 6, 1865.

5. Oscillations produced by gravitating disturbances of molecular elasticity. April 1, 1864.

6. Polarizing influences of thermal convection and radiation. April 15, 1864.

7. Resemblance between lunar-monthly and solar-daily barometric fluctuations. June 17, 1864. Proc. Roy. Soc., June 16, 1864.

8. Combined influences of rotation, variations of temperature and vapor, solar and lunar attraction, and æthereal oscillations "moving with the rapidity of light," upon barometric fluctuations. July 15, 1864.

9. Numerical relations of gravity and magnetism. Oct. 21, 1864.

10. Resemblance between solar and lunar magnetic and atmospheric tides. Dec. 16, 1864.

11. Inverse relation of specific magnetism to coefficients of dilatation. May 19, 1865.

12. Control of magnetic needles by artificial currents resembling the normal atmospheric currents. Oct. 6, 1865.

13. Relations of temperature to gravity and density. Sept. 21, 1866.

14. Relation of terrestrial gravity to the velocity of light. Sept. 21, 1866.

15. Approximate equality in the influence of solar radiation and of atmospheric currents upon fluctuations of temperature. Feb. 1, 1867.

16. Demonstration of lunar influence upon rainfall by comparing different periods of observation at the same station. Dec. 4, 1868; Aug. 18, 1871; etc.

17. Equivalence of solar "nascent" or dissociative velocity (1) to the velocity of light. April 2, 1869.

18. Cosmical relations of light to gravity. April 2, 1869.

19. Frequency of anticyclonic storms and of local cyclones in general anticyclones. March 3 and 17, 1871.

20. Resemblance of atmospheric, magnetic and oceanic currents. April 7, 1871.

21. Identity of law in cosmical and molecular forces. Feb. 16, 1872.

22. Simple relations of explosive energy to Sun's mass and distance. Feb. 16, 1872.

23. Ratio of *vis viva* of wave-propagation to *vis viva* of oscillating particles. Feb. 16, 1872.

24. Influence of centres of oscillation and of the factor of linear *vis viva*, 9, on planetary masses, distances and cyclical motions. March 1, 1872.

25. Tendency of elastic vibrations to produce harmonic vibrations, illustrated by terrestrial rotation, lunar distance, lunar revolution and the velocity of light. April 5, 1872.

26. The same tendency illustrated by apsidal and mean planetary positions and eccentricities, by solar, planetary and lunar rotations, and by the sun-spot-cycle of 11·07 years. May 16, 1872.

27. Nebula-rupturing velocities acquired by subsidence from ∞ to $\frac{n}{n+1}r$. Sept. 20, 1872.

28. Evidences of parabolic projection between Sun and the nearest fixed star. Sept. 20, 1872.

29. Harmony of stellar and planetary positions with Sun's gravitating reaction against luminous undulation. Sept. 20, 1872.

30. Lunar influence on rainfall less affected by locality than solar. Nov. 1, 1872.

31. Influence of the circular ratio, π, upon planetary positions. Feb. 7, 1873.

32. Increased symmetry introduced by the supposed failure of "Bode's Law." March 7, 1873.

33. Musical intervals in the Fraunhofer lines. March 21, 1873.

34. Musical intervals in planetary positions. April 4, 1873.

35. Harmonic indications of intra-Mercurial planets. May 2, 1873.

36. Harmonic correlations of planetary mass. May 2, 1873.

37. Confirmation of harmonic prediction. Oct. 3, 1873.

38. The harmonic planetary progression closer than any other that has been pointed out. Oct. 3, 1873.

39. Mean proportionality of perihelion parabolic velocity between normal velocity of solar oscillation and the velocity of light. May 15, 1874.

40. Harmonic grouping of planets into pairs. May 15, 1874.

41. Harmonic influence of the centre of gravity of Sun and Jupiter on planetary masses, positions and motions. May 15, 1874.

42. Influence of luminous undulation on the secular eccentricity of Jupiter, terrestrial gravity and cosmical masses. Jan. 1, 1875.

43. Analogous equations in general physics, electricity, chemistry and cosmogony. June 18, 1875.

44. Circular ratio of velocity of dissociation to velocity of cohesion. June, 18, 1875.

45. Estimate of Sun's mass and distance from the tidal relations of magnetism and gravitation. June 18, 1875.

46. Combined planetary evidences of Fourier's theorem and of a universal elastic medium. Aug. 20, 1875.

47. Different atmospheric and nucleal rupturing tendencies in Herschel's statement of the nebular hypothesis, and consequent misapprehension of the laws of nucleal condensation. Sept. 17, 1875.

48. The nucleal radius, in a rotating condensing nebula, varies as the $\frac{3}{4}$ power of Laplace's limiting radius, or radius of possible atmosphere. Sept. 17, 1875.

49. Mathematical deduction of the ratio of heat under constant

volume to heat under constant pressure from the *vis viva* of gaseous volume and of uniform velocity. Dec. 3, 1875.

50. Harmonics of products and powers of planetary mass and distance. Dec. 3, 1875.

51. Various confirmations of Herschel's theory of subsidence. April 21, 1876.

52. Evidences of parabolic influence in chemical atomicity. Feb. 2, 1877.

53. Various nebula-rupturing tendencies of "subsidence." July 20, 1877.

54. Identity of law in spectral lines and planetary arrangement. Aug. 24, 1877.

55. Confirmation of Herschel's nebular theory by the moons of Mars. Jan. 18, 1878.

56. Harmonic wave-lengths in chemical elements. Jan. 18, 1878.

57. Harmonic arrangement of satellite-systems. Jan. 18, 1878.

58. Relation of planetary positions to the solar modulus of light. March 1, 1878.

59. The centre of greatest planetary condensation is in Earth's orbit; the nebular centre of planetary inertia, in Saturn's orbit; the centre of the Neptuno-Uranian nebula, in Jupiter's orbit. March 1, 1878.

60. Nine intra-Mercurial harmonic positions showing tendencies to synchronous oscillation and confirming harmonic prediction. Oct. 4, 1878.

61. Two additional confirmations of harmonic prediction. Feb. 21, 1879.

62. Harmonics of Lockyer's "basic lines." April 4, 1879.

63. Harmonic relations of the terrestrial day to the year. Dec. 19, 1879.

64. Relations between cosmical masses and positions. Jan. 2, 1880.

65. Various planetary relations between gravitating and luminous velocities. March 19, 1880.

66. Relation of ocean temperatures to Joule's equivalent. April 16, 1880.

67. Relations of chemical affinity to luminous and cosmical energies. April 16, 1880.

68. Evidences of subsidence at the outer surface, and of rupturing

oscillation at the inner surface, of the two primitive planetary belts. Jan. 7, 1881.

69. Centrifugal as well as centripetal influence of paraboloidal projection between Sun and stars. Jan. 7, 1881.

70. Ratio of æthereal density to the density of hydrogen, deduced from the equality of action and reaction. Jan. 7, 1881.

71. Relation of masses, times and distances to luminous undulation. Jan. 21, 1881.

72. Three orders of harmonic spectra. April 15, 1881.

73. Cosmical significance of the corona line. April 15, 1881.

74. Stellar relations of the corona line and of mass. June 17, 1881.

75. Mutual convertibility of various natural physical units. June 17, 1881.

76. Relation of solar temperature to the natural terrestrial thermal unit. June 17, 1881.

77. Photo-dynamic and thermo-dynamic relations of the solar-stellar paraboloid. June 17, 1881.

78. Relations of Sun, Jupiter and Earth to photo-dynamic energy at the centre of greatest condensation. June 17, 1881.

In addition to the foregoing, there are more than two hundred subordinate facts which were discovered by studying the laws of æthereal action and reaction. A list of the papers in which they are enumerated was communicated to the American Philosophical Society, Nov. 5, 1880.

Radiophony.—E. Mercadier gives this name to the phenomenon discovered by Bell, in which an intermittent radiation of a definite period produces a sound of the same period. He has arrived by experiment at the following results: 1. Radiophony does not appear to be an effect of the receiving plate vibrating transversely, like an ordinary vibrating plate. 2. The nature of the molecules of the receiver and their mode of aggregation do not appear to exercise a predominant influence upon the nature of the sounds produced. 3. The sounds result from the direct action of the radiations upon the receivers. 4. The phenomenon seems to result principally from an action upon the surface of the receiver. 5. The radiophonic effects are relatively very intense. 6. They appear to be produced principally by calorific radiations or waves of great length.—*Compt. Rend.* C.

THE SCIENTIFIC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING.

By Prof. W. Grylls Adams, F.R.S.

A series of "Cantor Lectures" delivered before the Society of Arts, London, 1881.

It has been well said that rarely does a great discovery, as soon as it is made, at once begin to furnish the results which follow as a natural consequence from it. Nearly all important discoveries pass through a stage of neglect or obscurity. Either the public attention is already pre-occupied, or the discoveries come at a time when the public are not prepared for them, and they are disregarded and may even disappear with their authors for a time, to come forward again with fresh force in after years, when the world is more in tune to receive them. Sometimes they pass through a stage of quiet development in the laboratory; laws are established, apparatus is devised to prove them, attention is drawn to them, public spirit is awakened and from the higher level of the great discoverer flow new facts and new inventions, with astonishing rapidity, in many channels; the potential energy of the discoverer is transformed into energy of action in many directions, with more or less efficiency according to the retarding state of the medium through which that action takes place.

The progress of electrical science in its several branches will afford abundant instances of these several stages. If, for instance, we regard the progress of telegraphy, we find that Sir Francis Ronalds, in 1816, showed that electricity could be practically used for conveying messages over long distances; yet so little notice is taken of his discoveries by the public and by the government, who were no longer in need (so they said) of telegraphs after the battle of Waterloo, that he is almost driven to despair, and speaks of "taking leave of a science which once afforded him a favorite source of amusement," and of "bidding a cordial adieu to electricity."

It is remarkable that he should have said sixty years ago, "Let us have electrical conversation offices communicating with each other all over the kingdom," and yet the electric telegraph was not established until 20 years after (1837), and we are only now arriving at the system of telephonic exchanges.

In a private letter, written on his 72d birthday, in 1860, he says, "If the electric telegraph of 1816 had been fairly examined, an effective instrument might have been in the hands of the government and, after Dr. Oersted's experiments (in 1820), an improved telegraph might have been in their hands."

We shall also see other instances in connection with the special subject of my lectures, in which discoveries are neglected and passed by because, as we say, the discoverers were men who were in advance of their time, and in some cases the same discoveries are again made, and become known under another name. In 1815, Sir Francis Ronalds constructed an electric engine, which was set in motion by means of Singer's electric columns, and as late as 1851 this engine was still in working order, when it was, I believe, at the Kew observatory.

In 1813, *i. e.*, when Ronalds was experimenting on the electric telegraph at Hammersmith, by means of his registering pith-ball electrometers, Sir Humphry Davy produced the electric light between two carbons, which were joined to the two poles of a powerful battery.

The following is a description of Sir Humphry Davy's experiments with the electric light:

"Mr. Pepys having had the goodness to charge the great battery of the London Institution, consisting of 2000 double plates of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid and 25 parts of sulphuric acid, so as to make an arc or column of electric light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced, and a powerful magnet being presented to this arc or column, having its pole at a very acute angle to it, the arc or column was attracted or repelled with a rotatory motion, or made to revolve, by placing the pole in different positions, being repelled when the negative pole was on the right hand by the north pole of the magnet and attracted by the south pole."

With a few cells of some of the more powerful batteries, such as Grove's or Bunsen's, or the bichromate of potash battery, we may readily reproduce the celebrated experiment of Sir Humphry Davy.

When Davy discovered the electric light in 1813, little was known of voltaic electricity, except that the current decomposed salts, and was always accompanied by chemical action in the battery. Davy had obtained the metals potassium, sodium, barium, strontium, cal-

cium and magnesium by the electric current. The relation between electricity and magnetism was still unknown.

In the year 1820, Oersted first observed the action of a current of electricity on the magnetic needles, and thus gave a very ready method of comparing the effects of different currents, by balancing these effects on the needles against the effect of the earth's horizontal magnetic force. In the same year, Ampère discovered the law of the action of the current on the magnetic needles, and propounded his celebrated theory of magnets and of terrestrial magnetism. According to this theory, every particle of a piece of steel which forms the magnet has currents of electricity circulating round it in the same direction, and the magnetism of the magnet is only the resultant action of all these currents taken through the whole of the piece of steel. Thus magnetism is the resultant action of electric currents.

Ampère's experiments (which were repeated in the lecture) showed the mutual attraction and repulsion of parallel currents and of currents and magnets; also that a current in a solenoid or in a flat coil acts as a magnet; and that a hollow coil carrying a current attracts a core of soft iron and holds it up.

These elementary experiments are now very simple, and they may be very well known; but, as we shall see in the lectures which are to follow, some of these early and simple devices are found to be among the most efficient for controlling and regulating the current and steadying the light in some of the best electric lamps. Whilst Ampère was developing Oersted's experiment in one direction, Schweigger, in the same year (1820) employed it for the comparison of currents and invented the galvanometer. Then followed improved galvanometers by Becquerel and others; and, in 1827, Ohm gave his simple theory of the action of batteries, which was deduced from Volta's principle, and this has formed the groundwork of all later investigations of the subject.

If we consider these simple experiments of Oersted and Ampère in their relation to the now well-established principles of conservation of energy, we may arrive at some important conclusions which again are fully borne out by experiment. Thus, when a current of electricity passes along a wire in Oersted's experiment, part of its energy is spent in overcoming the resistance of the wire and another part is spent in causing the motion of the magnetic needle, *i. e.*, in doing work upon it in opposition to the pull of the magnetic force of the

earth upon it. This part of the energy, which is spent in twisting the magnetic needle about the axis, leaves less energy to be spent in producing the current, and so there is less current passing in the wire when the magnet is in the act of being deflected than there is in the same wire when there is no magnet. When the magnet is held at rest, or when it has settled into its position of rest, there is no longer any energy spent in keeping it there, and the full current again passes in the wire. Thus a current which deflects a magnet is itself diminished by that motion of the magnet. Take, again, any of the simple experiments of Ampère on the mutual action of currents of electricity upon one another and the motion of conductors carrying those currents. There is a conversion of energy of the current into motion of the conductor carrying the current in parallel wires attracting one another, and hence there is less current in the wire while the motion is actually taking place. Thus the approach of two wires carrying currents of electricity diminishes the currents flowing in the wires.

Now if the approach of these wires diminishes the currents in them, then the separation of the wires may be expected to increase the currents flowing in them, for in the separation work is done in the opposite direction. Hence, in the alternate approach and separation of the wires as they oscillate, the currents are diminished and increased alternately. If there be no current at all in one of the wires, then the separation of the two wires will give a current in the one which had no current in it, and the approach of the wires will give a current in the opposite direction. Thus we are led, by the well-known principles of energy, to results which are well known to be true by experiment, that the separation of the parallel wires, one of which is carrying a current of electricity, produces in the other wire a current of electricity in the same direction or a direct current, and their approach produces a current of electricity in the opposite direction, or an inverse current.

Let us pursue this relation of the principles of energy to the effects produced by electric currents a little further. With two parallel currents in two approaching wires, the amount of energy used up, and therefore producing no effect in the shape of current, depends on the amount required or expended in bringing the wires together. This depends on the rate at which they approach one another. The more rapidly they approach, the more energy is consumed, and the more the currents in the wires are diminished.

In the same way, if the wires are separating from one another, the currents are increased by an amount which depends upon the rate of their separation. Hence, not only their relative motion, but the rate at which they approach and recede from one another, will determine the changes produced in the electric currents in them.

As in the case of bodies falling under the action of the force of gravity, the amount of energy expended is measured by the square of the velocity of falling, so, in the case of currents of electricity in wires approaching one another, the energy expended is measured by the square of the velocity of approach; so that the alteration in the current takes place more and more rapidly as the rate of approach is increased. If, again, we apply this to the case where one of the wires has a current in it, and a second parallel wire has no current in it as long as it remains at rest, then the amount or strength of the direct current in the second wire will increase at a more and more rapid rate as the velocity of the separation of the wires is increased, and the strength of the inverse current in the second wire will increase at a more and more rapid rate as the velocity of approach of the wires is increased.

Here, then, we have the laws of the production of induced currents deduced according to the principles of energy from the relative motion of parallel currents, discovered by Ampère. These laws are of such importance in connection with the subject of these lectures, that I shall illustrate them a little farther by a few simple experiments, showing the effect of rate of approach or separation on the induction current.

Instead of actually removing the coils, if the current in the primary circuit be diminished, the effect is the same as if a wire carrying a part of it had been taken away, and so there is a direct current induced in the secondary wire, whereas, if the current in the primary wire be increased, the effect is the same as if a wire carrying the additional current had approached, and so an inverse current is induced in the secondary wire.

We shall get the greatest rate of separation by suddenly stopping the current in or breaking the primary current, and the greatest rate of approach by suddenly joining or making the current flow in the primary circuit. Hence, breaking the primary circuit produces a very intense rush of electricity, giving a direct current of great intensity in

the secondary wire; and making the primary circuit gives a powerful inverse current in the secondary wire.

Having shown the relation between the production of currents of electricity by induction and the experiments of Ampère, as we should regard them at the present day in their relation to the principles of energy, let us now consider for a few minutes the way in which they were first produced.

It is now just fifty years ago since Faraday communicated to the Royal Society his first series of papers, entitled: (1.) On "The Induction of Electric Currents;" (2.) on "The Evolution of Electricity from Magnetism;" (3.) on "A New Electrical Condition of Matter;" and (4.) on "Arago's Magnetic Phenomena."

Little did he imagine the marvelous results that were to flow from his experiments when he wrote, at the beginning of that communication, that he had been stimulated to investigate experimentally the inductive effects of electric currents, with the view of elucidating Ampère's beautiful theory of magnetism, and, in the hope of obtaining electricity from ordinary magnetism. In those papers he describes most minutely the details of his experiments, and unfolds, step by step, the laws of an induction current in the helix of wire, called B, placed near to another helix, called A, carrying a voltaic current. That, as long as a steady current was maintained in A, there was no current induced in B; that, on making contact in A, or on approaching the wires, there was a momentary inverse current in B, and, on breaking contact in A, or on separating the wires, there was direct induced current in B; that, as this current was of the nature of an electric wave, like the shock of a Leyden jar, it might magnetize a steel needle, although it produced slight effect on a galvanometer, and how his expectation was confirmed, and that the needle was magnetized opposite ways on making and on breaking contact.

Then, in his evolution of electricity from magnetism, he gives an account of the greatly increased effects on introducing soft iron cores into his helices of wire, and shows that similar effects are obtained by using ordinary magnets in place of a helix carrying a battery current round an iron core, *i. e.*, in place of an electro-magnet. Also, in place of a cylinder of iron in a helix of wire, he uses a welded soft iron ring, 6 inches in diameter, and $\frac{7}{8}$ of an inch in thickness, with helices wound round it—in fact, what would now be called a Gramme ring—and remarks that the iron cylinder arrangement was not so powerful

as the ring arrangement. Placing the core within the helix, he magnetizes it by bringing permanent magnets in contact with its ends, and observes "a deflection which indicates an induced current of electricity in the opposite direction to that fitted to form a magnet, having the same polarity as that really produced by contact with the bar magnets." Such a current would have converted the cylinder into a magnet of the opposite kind to that formed by contact with the poles A and B, and such a current moves in the opposite direction to the currents which in Ampère's beautiful theory are considered as constituting a magnet in the position figured. On bringing the bar in contact with the poles of the magnet, a current is induced in the wire in the direction indicated in the figure.

He then describes the experiment of introducing a magnet with a coil of wire, and shows that the same current is obtained whether the marked end of the magnet be introduced at one end of the coil or the unmarked end introduced at the other, and that a current is produced in the opposite direction to the former on withdrawing the magnet from either end.

Then after describing the method of producing his induction spark, and also muscular contractions in a frog by means of a loadstone and coils, which were lent to him from King's College, and remarking that the intensity of the effect produced depends upon the rate of separation of the coil from the polls of the loadstone, he concludes this section thus: "An agent which is conducted along metallic wires in the manner described, which, whilst so passing, possesses the peculiar magnetic actions and force of a current of electricity, which can agitate and convulse the limbs of a frog, and which finally can produce a spark, can only be electricity."

One other of the discoveries of Faraday, made in that memorable year 1831, we shall find to be of great importance in magneto-electric machines, viz., the difference of time between induction by a battery current in a coil and induction by a magnet, which requires a considerable interval of time to get up to its full strength. Faraday accounted for this retardation by supposing that there is a redistribution of the Ampèrian currents in the iron itself, so that the magnet requires time to rise to its full power.

We may well consider for a while the work of such a man, who, fifty years ago, in his first series of papers to the Royal Society, could establish so many laws of magnetic and current induction, and who

made possible the rapid development which is now going on in the science of electricity, and especially in the production of magneto- and dynamo-machines.

After the discovery of the principles which I explained in the last lecture, and the method of producing currents of electricity by the inductive action of magnets, or currents in motion, the laws of these currents were being developed; but, for twenty years after Faraday's discovery in 1831 that the sudden removal of a coil of wire from the pole of a magnet gave rise to a current, nothing was done to apply these laws for the purposes of electric lighting. Voltaic batteries were being improved, and the more constant and more powerful batteries of Daniell, and Grove, and Bunsen were discovered, and these were the sources employed to produce the more powerful currents of electricity. In this country Grove's battery was the favorite; and in our laboratories we may say that, up to the present time, the 40 or 50 cells of Grove have always been used to give us the electric light.

When used for optical experiments in the laboratory, the source of light should be as steady as possible, and as the carbons burn away, their points should be continually brought to the same position; hence the elaborate arrangements of wheel work and electro-magnets devised by Staite, in 1847, and by Foucault, which have reached very great perfection in the hands of Duboscq.

One of the carbons, that connected with the positive pole of the battery, burns away twice as fast as the other, and hence the wheel work must be adapted for feeding these carbons automatically at the proper rate.

When the current flows always in the same direction in the arc, as in this case where Grove's battery is used, and in all cases where the magneto-machine is adapted for producing continuous currents, the positive charcoal point or carbon becomes hollow, and wears away more rapidly; and the negative carbon becomes pointed, and wears away about half as fast.

In the Duboscq lamp, the positions of the points of the carbon are kept as nearly as possible the same; the carbons are moved towards one another by means of a drum carrying two wheels, whose diameters are as two to one, which move two racks, which carry the carbons towards one another. This lamp is far too complicated and delicate in its mechanism to use with magneto-electric machines, and

therefore the system adopted in it, for regulating the current and regulating the carbons, requires considerable modification, before this lamp can be adapted for general use for the purposes of electric lighting. It is especially adapted for use with optical apparatus, and for showing by projection on the screen the special characteristics of the arc formed by the glowing gases of various substances. We may use it now for showing the character of the arc formed by silver converted into a glowing gas by the intense heat of the arc. This arc shows us that silver is rich in the violet or chemical rays, and points to the reason why the salts of silver are of so much use in photography.

Electric Regulators or Governors.

By the laws of Ohm we get the relation between the electro-motive force, the current, and the resistance expressed by the statement:

The product of the current by the resistance in a circuit is equal to the electromotive force in that circuit, or $E = C(R + r)$.

Regulators may act so as to control:

1. The electromotive force and internal resistance of the battery or dynamo-electric machine.

2. Or they may control the useful resistance in the circuit.

3. Or they may control the external resistance, which does not produce useful work.

Regulators which control the current by altering the electromotive force or internal resistance of the source of electricity, so as to counterbalance other distributing effects, are not practically of much importance, and so need not detain us long. Suppose, for instance, that an increase of current acted on a rotating governor in such a way as to raise the plates out of the battery, thereby increasing the internal resistance, this would diminish the currents which would so react on the governor, and again lower the plates. Or suppose the current passes round a coil which is set with its axis vertical, and that an iron rod supports the carbon and zinc in a bichromate of potash battery, and passes into the axis of the coil, then, as the current increases, the coil draws up the iron core and the battery plates, and increases the internal resistance, which diminishes the current.

Regulators which act on the useful resistance in the circuit, *i. e.*, in the case of electric lighting, the different arrangements made in electric lamps for producing steady currents between the carbon points,

by keeping them the same distance apart, are very numerous, and I hope to treat of them in a future lecture.

Another kind of regulator controls the current by varying the external resistance of the circuit, so that when the current increases an additional resistance is thrown into the circuit, and when the current diminishes the external resistance is diminished. If a machine is working with the greatest efficiency, then the addition of external resistance will diminish that efficiency, so that a regulator which varies the external resistance diminishes the efficiency of the machine in order to maintain a steady current.

There are many ways of varying the external resistance of a circuit; for instance, a rheostat set in action by clockwork, which is started by an armature of an electro-magnet placed in the circuit. If, for instance, as the current is weakened, the armature of the electro-magnet falls and releases a wheel of a clockwork arrangement, which diminishes the resistance by unwinding the wire of the rheostat. The methods which have been employed have been gradually simplified, and it is found that the simplest means are at the same time the most efficient. For weak currents, Edison's system, whereby a greater or less pressure on powdered carbon increases or diminishes its conductivity, has been employed. Edison also devised a regulator or shunt for the current, by the expanding of a platinum spiral wire placed in the lamp which short-circuited the current on reaching a certain definite temperature. Suppose, for instance, that an arrangement is made by which, when a current increases, part of it is sent through an electro-magnet, which draws up an arm so as to break the direct circuit, and send all the current through the electro-magnet, the resistance of the coil of the electro-magnet reduces the current, the arm falls, and again the current passes through the direct circuit.

Lane-Fox Regulator.

The regulator is an electro-magnet of very high resistance, which takes a branch of the current and acts on one end of a lever. The other end of the lever makes contact with one or other of two pins, which are connected with one or other of two coils forming electro-magnets, called respectively H and K, which govern either the throttle valve of the steam engine, or which may be made to introduce extra resistance by sliding contact over the wires of a rheostat. When the lever touches one of the pins, a current passes through the lever to

the electro-magnet H and turns the arm in one direction, and when it touches the other pin, the arm turns in the opposite direction, so that in one case the resistance of the circuit is increased and in the other it is diminished.

Siemens Regulator.

By means of the expansion of a fine strip of mild steel or fused iron stretched between two points, when the electric current passes through it, a vertical spindle, supporting a circular metallic disc, with platinum contacts on its upper surface, is lowered, so that the contacts of platinum, with certain points in a helical rheostat, are broken one by one, and at each break an additional portion of the rheostat is thrown into the circuit, and so the excess of current is checked. For the normal current the rheostat is out of circuit, but an excess of current heats the wire, lowers the platinum contacts, and brings more or less of the rheostat into the circuit. For small variations of current, the change of current is nearly proportional to the change of temperature in the strip.

Methods of Measuring Electric Currents.

There are four principal methods of measuring powerful electric currents.

1. *The Galvanometer Method.*—With a tangent galvanometer of small resistance, it is necessary to bring the deflections to about 45° by a shunt of very small resistance, which sends only a very small part of the current through the galvanometer. Here there is a liability to error in the measurement of the resistance of the shunt. The objection to this method is that a small quantity is measured by the galvanometer, and the error of the observation is multiplied, it may be a thousandfold, or even very much more, in order to arrive at any idea at all of the total current flowing in the principal circuit. I can only compare the method to an attempt to estimate the flight of starlings, or of a covey of birds, by measuring or marking, as accurately as possible, the flight of one particular bird which has been separated from the rest, and which is assumed to travel at the same rate, no matter what obstructions or attractive influences may have come across its path. At the same time, the difficulties of these measurements are so great that any method may be of great service, and this method has been employed by several observers with good results.

For strong currents, instead of a tangent galvanometer, Professor Trowbridge, of Harvard University, employs a galvanometer in which

the coil carrying the current is capable of turning about a horizontal axis, passing through the centre of the needle. When the coil is vertical the instrument is a tangent galvanometer; but on turning the coil through any angle, the part of the current which deflects the needle will be diminished: by this the current has very little effect in turning the needle when the coil is near the horizontal position.

2. *The Electrometer Method.*—The difference of potential between two points in a closed circuit may be measured directly, either by an electrometer like Thomson's quadrant electrometer, or by balancing the electromotive force between the two points, by a battery of the same electromotive force, in the circuits of which a galvanometer is placed, in which case the electromotive force is found by finding at what points the wires from the battery shall be attached, so that no current shall pass through the galvanometer. An instrument for the purpose I have described is Clark's potentiometer. This method has been employed Dr. Hopkinson and by others, to determine the current produced by a Siemens machine for the electric light. It has also been applied by Mr. Joubert and others to determine the current required to make the Jablochkoff candle burn at its best, and also for the estimation of the current given both by continuous current machines and also by alternate current machines for producing the electric arc.

3. *Method of Using Thomson's Electrometer.*—If V be the potential of the needle, and V_1 V_2 the potential of the quadrants and d the deviation of the needle, k being a constant, then

$$d = k\,(V_1 - V_2)\left(V - \frac{V_1 + V_2}{2}\right)$$

But, if the needle and one pair of quadrants be connected,

$$d = \frac{k}{2}\,(V_1 - V_2)^2, \text{ where } V = V_1$$

so that the deviation is proportional to the square of the difference of potential, and is therefore independent of the direction of the current.

By means of two electrometers so arranged, the current and energy expended between any two points of a circuit may be at once determined. First consider the case of a continuous current. Let one electrometer have its poles attached to two points, A and B, of the circuit where the potentials are V_1 and V_2, having a resistance, r_1, between them and a current, C, then

$$Cr_1 = V_1 - V_2.$$

Now take two other points, C and D, in the same circuit, which

have a difference of potential, $V_3 - V_4$, and which have an electromotive force, E, as well as a resistance, r_2, between them, then

$$E + Cr_2 = V_3 - V_4.$$

Then the energy expended between these two points is

$$C(E + Cr_2) = \frac{(V_1 - V_2)(V_3 - V_4)}{r_1},$$

so that the deflection of the two electrometers will at once give the

$$\text{Energy expended} = \frac{2}{r_1}\sqrt{\frac{d_1}{k_1}\frac{d_2}{k_2}}.$$

One electrometer gives the current, and the two together give the work expended.

If, instead of a continuous current, we have alternate currents succeeding one another at intervals which are very short compared with the time of oscillation of the needle, then the needle will remain steadily deflected at a deviation proportional to the mean value of the square of the difference of potential:

$$d = \frac{k}{2}(V_1 - V_2)^2.$$

In this case the difference of potential must be measured absolutely at the same instant. This may be done by placing two contact breakers on the revolving axis of the dynamo machine, so arranged that by both of them contact is made and broken at the same instant some 20,000 times in a second, then the two electrometers will give the desired results. In place of the electrometer which is employed to give the strength of current, a galvanometer may be employed, as in Clark's potentiometer, in which case it is only necessary to have one contact breaker, and so the arrangements are more easily made.

The law of variation of the current in alternate current machines may be obtained by the law of variation of the resistance for different periods of contact, *i. e.*, for different phases of rotation of the contact breaker. Thus the different phases of the current may be studied by dividing the period into a certain number of equal parts, and, by means of the contact breaker, making contact only at the same phase of the period of revolution.

The intensity of the light at the different phases of the period may also be studied and measured by means of revolving discs with holes in them, arranged so as to let light through only at the proper instant. The result of experiments is that the law of increase of current in alternate current machines is the law of simple harmonic motion, but

the maximum effect does not occur at the normal position, but is displaced in the direction of the motion. Experiments, of which a full account has been given in *La Lumière Electrique*, have shown that even with coils alone there is a retardation of the inductive action arising from the induction of a current on itself. The retardation is about $\frac{1}{8}$ of the whole period, even with a bobbin without a soft iron core, and this displacement is independent of the velocity, and rigorously the same for 400, 700 or 1000 turns in a minute. In the case of magneto-electric machines, there is a retardation which is usually attributed to the retardation of magnetization of the magnet, but that does not apply to the case of coils, in which case the retardation depends on the self-induction of the current. By the above method, the fall of potential between the carbons in the arc may be measured at different phases of the period of revolution. Thus it is found that at the instant when the circuit is made there is no difference of potential, but in an indefinitely short time the electromotive force rises to 40 or 45 volts, and remains at that value almost without change until the current again becomes very feeble, and then the electromotive force suddenly falls. The difference of potential in the arc seems to be constant within very wide limits for the values of the current. The conclusions arrived at by M. Joubert are that the resistance of the arc is very small; that it varies with temperature and diminishes as the temperature increases. The difference of potential between the two carbons is due to an electromotive force, which is independent of the current, and which is estimated by him at 30 volts. Particles pass between the carbons just as between the electrodes in a voltameter. There seems to be an action like polarization, and the work done depends only upon, and is proportional to, the quantity of electricity which passes.

Another method of measuring the currents which is applicable to both continuous currents and to obtain the average values of alternate currents, depends on the development of heat in an electric circuit. It was shown by Joule, and follows from the law of transformation of energy, that the heat developed by a current, C, in a resistance, r, in time, t, is $C^2 r t$, hence the current, C, may be measured by the quantity of heat received in a given time by water, in which a resistance, r, is immersed. This method has been employed by Dr. Siemens, and was one of the methods employed by Dr. Hopkinson in measuring electric light currents.

In his experiments Joule inserted a wire of known resistance in a given quantity of water placed in a calorimeter, and measured the change of temperature of the water, and also measured the current, and found that $H = C^2 r t$, where H is the quantity of heat produced by the current.

The Electro-Dynamometer Method.—There is still another method of measuring currents, which Maxwell says "is probably the best fitted for absolute measurements."

In Weber's electro-dynamometer one coil is suspended within another by means of two fine wires, through which the current is led to the suspended coil. This arrangement is not suitable for powerful currents, because shunts become necessary, and because the suspended wires become heated.

An electro-dynamometer on the same principle has been devised by Dr. C. W. Siemens, who has kindly lent me one of his instruments for these lectures.

Attached to a binding screw is one end of a fixed coil or bobbin, across which a single turn of copper wire with its ends dipping into two mercury cups, is freely suspended in a vertical plane, so as to turn about a vertical axis. One of the mercury cups is electrically connected with a binding screw, and the other with one end of the fixed coil, so that the same current passes through the fixed coil and suspended wire. When the current passes, the suspended wire tends to turn about a vertical axis, but its motion is counteracted by a torsion spring, to one end of which the wire is attached, the other end being fixed to a socket carrying an index, which must be moved over a graduated circle, so as to bring the suspended wire back to its initial position. The action between the currents in the coils, and therefore the torsion which measures it, is proportional to the square of the current. By increasing the number of turns in the fixed coil, and having only one turn in the suspended wire, the action of the earth's magnetism on the current in the suspended wire may be neglected in comparison with the action of the fixed coil upon it, so that the position of the plane of the magnetic meridian at the place of observation may be disregarded in using this instrument. There are usually two fixed coils or bobbins; one coil, attached to a binding screw, being of a small number of turns of thick wire for continuous currents, and the other, attached to a similar binding screw, being much longer and

finer, for use with machines giving alternate currents or currents of high tension.

An electro-dynamometer has also been devised by Professor Trowbridge, of Harvard University, in which there are two large fixed coils made from copper bands, between which is suspended from a torsion head a small coil with mercury connections, so that all the current passes through each coil. This instrument has been improved by Mr. W. N. Hill, who limits the swing of the central coil, and measures the current by bringing the coil back to its zero position, by balancing the force of repulsion of the coils by weights placed in two scale pans, one on each side of the instrument, so as to balance the action of the current by torsion of the suspending wire. If C be the current, $w\,l$ the twisting moment of the weight, w, G and g the constants for the two coils, and k the constant of the instrument,

$$\text{then } C^2 = \frac{l\,w}{k\,G\,g}$$

G and g are found by measurement, and k is found by comparison with another instrument. Through the kindness of Messrs. Elliott Brothers I have the opportunity of bringing before you an electro-dynamometer of this form with the latest improvements. This form of dynamometer is especially applicable for large currents, since the weights required to bring the deflection to zero increase as the square of the current, and so greater accuracy may be attained.

(To be continued.)

Mechanical Imitation of Electric and Magnetic Actions. —Chase's apparatus for imitating "lines of force" and showing a mechanical control of magnetic currents has furnished a precedent for many similar contrivances. One of the latest is that of M. C. A. Bjerknes, for the hydrodynamic imitation of electric and magnetic actions. The inventor proposes to use his apparatus not only for the investigation of known laws, but also for the discovery of laws and methods which have been hitherto unknown. He has been for some time occupied upon the study af oscillations of various kinds. His attention appears to have been turned in this direction by the acoustic attractions and repulsions which were experimentally examined by Guyot, Schellbach and Guthrie, and which have been partially studied mathematically by Sir Wm. Thomson.—*Comptes Rendus.* C.

INDIGO AND ITS ARTIFICIAL PRODUCTION.

By H. E. Roscoe, LL.D., F.R.S., President of the Chemical Society.

A paper read before the Royal Institution of Great Britain, Friday, May 27, 1881.

More than eleven years ago the speaker had the pleasure of bringing before this audience a discovery in synthetic chemistry of great interest and importance, viz., that of the artificial production of alizarin, the coloring substance of madder. To-day it is his privilege to point out the attainment of another equally striking case of synthesis, viz., the artificial formation of indigo. In this last instance, as in the former case, the world is indebted to German science, although to different individuals, for these interesting results, the synthesis of indigo having been achieved by Professor Adolf Baeyer, the worthy successor of the illustrious Liebig in the University of Munich. Here, then, we have another proof of the fact that the study of the most intricate problems of organic chemistry, and those which appear to many to be furthest removed from any practical application, are in reality capable of yielding results having an absolute value measured by hundreds of thousands of pounds.

In proof of this assertion, it is only necessary to mention that the value of the indigo imported into this country in the year 1879 reached the enormous sum of close on two millions sterling, whilst the total production of the world is assessed at twice that amount; so that if, as is certainly not impossible, artificial indigo can be prepared at a price which will compete with the native product, a wide field is indeed open to its manufacturers.

Indigo, as is well known, is a coloring matter which has attracted attention from very early times. Cloth dyed with indigo has been found in the old Egyptian tombs. The method of preparing and using this color is accurately described by both Pliny and Dioscorides, and the early inhabitants of these islands were well acquainted with indigo, which they obtained from the European indigo plant, *Isatis tinctoria*, the woad plant, or pastel. With this they dyed their garments and painted their skins. After the discovery of the passage to India by the Cape of Good Hope, the Eastern indigo, derived from various species of *Indigofera*, gradually displaced woad, as containing more of

the coloring matter. But this was not accomplished without great opposition from the European growers of woad; and severe enactments were promulgated against the introduction of the foreign coloring matter, an edict condemning to death persons "who used that pernicious drug called devil's food" being issued by Henry the Fourth of France. The chief source of Indian indigo is the *Indigofera tinctoria*, an herbaceous plant raised from seed which is sown in either spring or autumn. The plant grows with a single stalk to a height of about 3 feet 6 inches, and about the thickness of a finger. It is usually cut for the first time in June or July and a second or even a third cutting obtained later in the year. The value of the crop depends on the number of leaves which the plant puts forth, as it is in the leaves that the coloring principle is chiefly contained. Both the preparation of the coloring matter from the plant and its employment as a dye or as a paint are carried on at the present day exactly as they have been for ages past. The description of the processes given by Dioscorides and Pliny tally exactly with the crude mode of manufacture carried on in Bengal at the present day, as follows:

"The Bengal indigo factories usually contain two rows of vats, the bottom of one row being level with the top of the other. Each series numbers from fifteen to twenty, and each vat is about 7 yards square and 3 feet deep; they are built of brickwork, lined with stone or cement. About a hundred bundles of the cut indigo plants are placed in each vat in rows and pressed down with heavy pieces of wood; this is essential to the success of the operation. Water is then run in so as to completely submerge the plants, when a fermentation quickly ensues, which lasts from nine to fourteen hours, according to the temperature of the atmosphere. From time to time a small quantity of the liquor is taken from the bottom of the vat to see how the operation is proceeding. If the liquor has a pale yellow hue, the product obtained from it will be far richer in quality but not so abundant as if it had a golden-yellow appearance. The liquor is then run off into the lower vats, into which men enter and agitate it by means of bats or oars, or else mechanically by means of a dash-wheel, each vat requiring seventeen or eighteen workpeople, who are kept employed for three or four hours. During the operation, the yellow liquor assumes a greenish hue and the indigo separates in flakes. The liquor is then allowed to stand for an hour and the blue pulpy indigo is run into a separate vessel, after which it is pumped up into a pan and

boiled, in order to prevent a second fermentation, which would spoil the product by giving rise to a brown matter. The whole is then left to stand for twenty hours, when it is again boiled for three or four hours, after which it is run on to large filters, which are placed over vats of stonework about 7 yards long, 2 yards wide and 1 yard deep. The filters are made by placing bamboo canes across the vats, covering these with bass mats and over all stretching strong canvas. The greater part of the indigo remains under the form of a dark-blue or nearly black paste, which is introduced into small wooden frames having holes at the bottom and lined with strong canvas. A piece of canvas is then placed on the top of the frame, a perforated wooden cover, which fits into the box, put over it and the whole submitted to a gradual pressure. When as much of the water as possible has been squeezed out, the covers are removed and the indigo allowed to dry slowly in large drying sheds, from which light is carefully excluded. When dry, it is ready for the market. Each vat yields from 36 to 50 lbs. of indigo" (Grace-Calvert).

The same process carried out in the times of the Greeks is thus described by Dioscorides: "Indigo used in dyeing is a purple-colored froth formed at the top of the boiler; this is collected and dried by the manufacturer; that possessing a blue tint and being brittle is esteemed the most."

The identity of the blue coloring matter of woad and that of the Bengal plant was proved by Hellot and by Planer and Trommsdorff at the end of the last century. These latter chemists showed that the blue color of the woad can be sublimed and thus obtained in the pure state; a fact which was first mentioned in the case of indigo by O'Brien, in 1789, in his treatise on calico printing. Indigo thus purified is termed indigotin. It has been analyzed by various chemists, who ascertained that its composition may be most simply expressed by the formula C_8H_5NO.

Indigo is a blue powder, insoluble in water, alkalies, alcohol and most common liquids. In order to employ it as a dyeing agent it must be obtained in a form in which it can be fixed or firmly held on to the fibres of the cloth. This is always effected by virtue of a property possessed by indigo-blue of combining with hydrogen to form a colorless body, soluble in alkalies, known as indigo-white or reduced indigo, of which the simplest formula is C_8H_6NO. This substance rapidly absorbs oxygen from the air and passes into the blue

insoluble indigo which, being held in the fibre of the cloth, imparts to it a permanent blue dye. This reduction to white indigo may be effected in various ways. The old cold vat, or blue-dip vats as they are termed, consist of a mixture of indigo, slaked lime and green vitriol. The latter salt reduces the indigo and the white indigo dissolves in the lime water. This process of indigo dyeing is both expensive and troublesome, owing to loss of indigo and formation of gypsum, so that many plans have been proposed to remedy these evils.

Concerning the origin of indigo in the leaves of the *Indigofera*, various and contradictory views have been held. Some have supposed that blue indigo exists ready formed in the plant, others that white indigo is present which, on exposure to air, is converted into indigo-blue. Schunck has, however, proved beyond doubt that the woad plant (*Isatis tinctoria*), the *Indigofera tinctoria* of India and the Chinese and Japanese indigo plant (*Polygonum tinctorium*) contain neither indigo-blue nor white indigo ready formed. It is now known that by careful treatment the leaves of all these indigo-yielding plants can be shown to contain a colorless principle termed indican, and that this easily decomposes, yielding a sugar-like body and indigo-blue. That white indigo is not present in the leaves is proved by the fact that this compound requires an alkali to be present in order to bring it into solution, whereas the sap of plants is always acid. The decomposition is represented by Schunck as follows:

$$\underset{\text{Indican.}}{C_{26}H_{31}NO_{17}} + 2H_2O = \underset{\text{Indigotin.}}{C_8H_5NO} + \underset{\text{Indiglucin.}}{3C_6H_{10}O_6}.$$

So readily does this change from indican to indigo take place that bruising the leaf or exposing it to great cold is sufficient to produce a blue stain. Even after mere immersion in cold alcohol or ether, when the chlorophyl has been removed the leaves appear blue, and this has been taken to show the pre-existence of indigo in the plant. But these appearances are deceptive, for Schunck has proved that if boiling alcohol or ether be used, the whole of the color-producing body as well as the chlorophyl is removed, the leaves retaining only a faint yellow tinge, whilst the alcoholic extract contains no indigo-blue, but on adding an acid to this liquid the indican is decomposed and indigo-blue is formed.

Passing now to the more immediate subject of his discourse, the speaker again reminded his hearers that indigo was the second natural

coloring matter which has been artificially prepared; alizarin, the coloring matter of the madder root, being the first. As a rule, the simpler problems of synthetic chemistry are those to which solutions are the soonest found, and these instances form no exception to the rule. The synthetic production of indigo is a more difficult matter than the artificial formation of alizarin, and hence the speaker did not apologize for leading up to the complex through the more simple phenomenon.

When the ingenious Japanese workman, who had never seen a watch, had one given to him in order to make a duplicate, he took the only sensible course open to him, and carefully pulled the watch to pieces, to see how the various parts were connected together. Having once ascertained this, his task was a comparatively easy one, for he then had only to make the separate parts and fit them together, and he thus succeded so well in imitating the real article that no one could tell the difference. So it is with the chemist; until he knows how the compound is built up, that is, until he has ascertained its constitution, any attempt at synthesis is more like groping in the dark than like shaping the course by well-known landmarks into harbor.

In the case of alizarin it was easy to reduce it to its simplest terms and to show that the backbone of this coloring matter is anthracene, $C_{14}H_{10}$, a hydrocarbon found in coal-tar. This fact being ascertained, the next step was the further process of clothing the hydrocarbon by adding four atoms of oxygen and subtracting the two atoms of hydrogen present in excess, and this was soon successfully accomplished, so that now, as we know, artificial alizarin has excluded the natural coloring matter altogether.

$C_{14}H_{10}$.	$C_{14}H_6O_2(OH)_2$.
Anthracene.	Alizarin.

What now was the first step gained in our knowledge concerning the constitution of indigo, of which the simplest formula is C_8H_5NO?

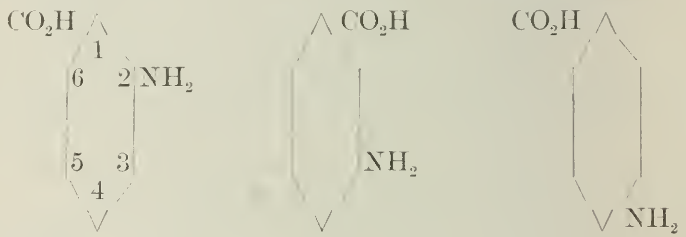
STEP No. 1.—This was made so long ago as 1840, when Fritsche proved that aniline, $C_6H_5NH_2$, can be obtained from indigo. The name for this now well-known substance is indeed derived from the Portuguese "anil," a word used to designate the blue color from indigo. This result of Fritsche's is of great importance, as showing that indigo is built up from the well-known benzine ring, C_6H_6, the skeleton of all the aromatic compounds and, moreover, that it contains an amido- group.

STEP NO. 2 was also made by Fritsche in the following year, when, by boiling indigo with soda and manganese dioxide, he obtained ortho-amido-benzoic acid or, as he then termed it, anthranilic acid. The following is the reaction which here occurs:

$$\underset{\text{Indigo.}}{C_8H_5NO}+O+2H_2O=\underset{\text{Ortho amido-benzoic acid.}}{C_7H_5NH_2O_2}+CH_2O_2.^*$$

What light does this fact shed upon the constitution of indigo? It shows (1) that one of the eight atoms of carbon in indigo can be readily separated from the rest; (2) that the carboxyl and the amido-group are in neighboring positions in the benzine ring, viz., 1 and 2. For we have three isomeric acids, of the following composition:

Ortho position. Meta position. Para position.



STEP NO. 3.—The next advance of importance in this somewhat complicated matter is the discovery by Erdmann and Laurent independently, that indigo on oxidation yields a crystalline body which, however, possesses no coloring power, to which they gave the name of isatin.

$$\underset{\text{Indigo.}}{C_8H_5NO}+O=\underset{\text{Isatin.}}{C_8H_5NO_2}.$$

STEP NO. 4.—The reverse of this action, viz., the reduction of isatin to indigo, was accomplished by Baeyer and Emmerling in 1870 and 1878, by acting with phosphorus pentachloride on isatin and by the reducing action of ammonium sulphide on the chloride thus formed.

Understanding now something of the structure and of the relationships of the body which we wish to build up, let us see how this edifice has, in fact, been reared. Three processes have been successfully employed for carrying out this object. But of these three, only one is of practical importance. A synthetic process may yield the wished-for result, but the labor incurred may be too great and the losses

* Böttinger, *Deut. Chem. Ges.*, 1877, i, 269.

during the campaign may be too severe to render it possible to repeat the operation with advantage on a large scale. Just as it costs, at the usual rate of wages, more than twenty shillings to wash a sovereign's worth of gold out of the Rhine sands, so that this employment is only carried on when all other trades fail.

For the sake of completeness let us, however, consider all three processes, although Nos. 1 and 2 are at present beyond the pale of practical schemes.

These three processes have certain points in common. (1) They all proceed from some compound containing the benzine nucleus. (2) They all start from compounds containing a nitrogen atom. (3) They all commence with an ortho- compound.

They differ from one another, inasmuch as process No. 1 starts from a compound containing 7 atoms of carbon (instead of 8), and to this, therefore, one more atom must be added; process No. 2, on the other hand, starts from a body which contains exactly the right number (8) of carbon atoms; whilst No. 3 commences with a compound in which 9 atoms of carbon are contained and from which, therefore, 1 atom has to be abstracted before indigo can be reached.

Process No. 1 (Kekulé—Claissen and Shadwell).—So long ago as 1869, Kekulé predicted the constitution of isatin and gave to it the formula which we now know that it possesses, viz.:

$$\begin{array}{ccc} & CO & \\ C_6H_4 & & CO. \\ & NH & \end{array}$$

Following up this view, Claissen and Shadwell, two of Kekulé's pupils, succeeded in preparing isatin and, therefore, indigo from ortho-nitro-benzoic acid.

The following are the steps in the ascent:

1. Ortho-nitro-benzoic acid acted on by phosphorus pentachloride yields the chloride, $C_6H_4(NO_2)COCl$.

2. This latter heated with silver cyanide yields the nitril, $C_6H_4(NO_2)CO.CN$.

3. On heating this with caustic potash it yields ortho-nitro-phenyl-glyoxylic acid, $C_6H_4(NH_2)CO.CO_2H$.

4. This is converted by nascent hydrogen into the amido- compound, $C_6H_4(NH_2)CO.CO_2H$.

5. And this loses water and yields isatin, $C_6H_4NH.CO.CO$.

(Q. E. D.)

The reasons why this process will not work on a large scale are patent to all those who have had even bowing acquaintance with such unpleasant and costly bodies as phosphorus pentachloride or cyanogen.

Process No. 2.—Baeyer's (1878) synthesis from ortho-nitro-phenyl-acetic acid.

This acid can be obtained synthetically from tolnol, and it is first converted into the amido- acid and which, like several ortho- compounds, loses water and is converted into a body called oxindol, from which isatin and, therefore, indigo can be obtained. The precise steps to be followed are:

1. Ortho-amido-phenyl-acetic yields oxindol:

$$C_6H_4\begin{cases}CH_2CO_2H\\NH_2\end{cases} = C_6H_4\begin{cases}CH^2\\NH\end{cases}CO + H_2O.$$

2. This on treatment with nitrous acid yields nitros-oxindol:

$$C_6H_4\begin{cases}C(NO)\\NH\end{cases}CO.$$

3. This, again, with nascent hydrogen gives amidoxindol:

$$C_6H_4\begin{cases}CH(NH_2)\\NH\end{cases}CO.$$

4. Which on oxidation gives isatin:

$$C_6H_4\begin{cases}CO\\CH\end{cases}CO.$$

(Q. E. D.)

This process, the feasibility of which had also been foreseen by Kekulé, is, however, not available as a practical scheme for various reasons.

Process No. 3.—This may be called the manufacturing process and was also proposed by Baeyer. It starts from cinnamic acid, a sub-

stance contained in gum benzoin, balsam of Peru and some few other aromatic bodies. These sources are, however, far too expensive to render this acid thus obtained available for manufacturing purposes. But Bertagnini, in 1856, had obtained cinnamic acid artificially from oil of bitter almonds, and other processes for the same purpose have since been carried out. Of these, that most likely to be widely adopted is the following practical modification, by Dr. Caro, of Mr. Perkin's beautiful synthesis of cinnamic acid :

1. $\underset{\text{Toluene.}}{C_6H_5CH_3} + 4Cl = \underset{\text{Benzylene Dichloride.}}{C_6H_5CHCl_2} + 2HCl.$

2. $\underset{\text{Benzylene Dichloride.}}{C_6H_5CHCl_2} + \underset{\text{Sodium Acetate.}}{2CH_3.CO.O.Na} =$

$$\underset{\text{Cinnamic Acid.}}{C_6H_5CH = CH.CO.OH.} + 2NaCl.$$

But why did Baeyer select this 9-carbon acid from which to prepare indigo? For this he had several reasons. In the first place, it had long been known that all indigo compounds when heated with zinc dust yield indol, C_8H_7N, a body which stands, therefore, to indigo in the same relation as anthracen to alizarin, and Baeyer and Emmerling had, so long ago as 1869, prepared this indol from ortho-nitro-cinnamic acid, thus: $C_8H_6(NO_2)CO_2H = C_8H_7N + O_2 + CO_2$.

Secondly, the ortho-nitro-cinnamic acid required (for we must remember that indigo is an ortho- compound and also contains nitrogen) can be readily prepared, and this itself again can be obtained on a large scale. Thirdly, this acid readily parts with one atom of carbon and thus renders possible its conversion into 8-carbon indigo.

The next steps in the process are (3) the formation of ortho-nitro-cinnamic acid, (4) the conversion of this into its dibromide, (5) the separation from this of the two molecules of hydrobromic acid, giving rise to ortho-nitro-phenyl-propiolic acid, and (6) lastly, the conversion of this latter into indigo by heating its alkaline solution with grape sugar, xanthate of soda or other reducing agent. These reactions are thus represented :

3. $\underset{\text{Cinnamic Acid.}}{C_6H_5CH = CHCOOH} \qquad \underset{\text{[illegible]}}{} \qquad \underset{\text{[illegible]}}{C_6H_4(NO_2)CH = CH.COOH.}$

In this process the para- acid is also obtained and, as this is useless for the manufacture of indigo, it has to be removed. This is effected by converting the acids into their ethyl-ethers which, possessing dif-

ferent degrees of solubility, can be readily separated from one another.

4. This is next converted into the dibromide,

$$C_6H_4(NO_2)CHBr.CHBrCOOH.$$

5. And by careful treatment with caustic soda this yields ortho-nitro-phenyl-propiolic acid, thus:

$$C_6H_4(NO_2)CHBrCHBrCOOH + 2NaOH =$$
$$= C_6H_4(NO_2)C_2.COOH + 2NaBr + 2H_2O.$$

6. $$n[\underset{\text{Ortho-nitro-phenyl-propiolic Acid.}}{C_6H_4(NO_2)C_2.COOH} + H_2 = \underset{\text{Indigotin.}}{C_8H_5NO} + CO_2 + H_2O].$$

(Q. E. D.)

The last of these reactions is in reality not so simple as the equation indicates. For only about 40 per cent. of indigo is obtained, whereas according to theory 68 per cent. should result. Indeed, although, as we have seen, indigo can be prepared by these three methods, chemists are as yet in doubt as to its molecular weight, the probability being that the molecule of indigo contains twice 16 atoms of carbon, or has the formula $4(C_8H_5NO)$ or $C_{32}H_{20}N_4O_4$. Still it must be remembered that, according to Sommaruga, the vapor-density of indigo is 9·45, a number corresponding to the simpler formula $C_{16}H_{10}N_2O_2$.

The artificial production of indigo may even now be said to be within reasonable distance of commercial success, for the ortho-nitro-phenyl-propiolic acid, the colorless substance which on treatment with a reducing agent yields indigo-blue, is already in the hands of the Manchester calico printers and is furnished by the Baden Company for alkali and aniline colors at the price of 6s. per lb. for a paste containing 25 per cent. of the dry acid.

With regard to the nature of the competition between the artificial and the natural coloring matters, it is necessary to say a few words. In the first place, the present price at which the manufacturers are able to sell their propiolic acid is 50s. per kilo. But 100 parts of this can only yield, according to theory, 68·58 parts of indigo-blue, so that the price of the artificial (being 73s. per kilo.) is more than twice that of the pure natural color. Hence, competition with the natural dye-stuff is not to be thought of until the makers can reduce the price of dry propiolic acid to 20s. per kilo., and also obtain a theoretical yield from their acid. This may, or it may not, be some day accomplished, but at present it will not pay to produce indigo from nitro-phenyl-propiolic acid. Nevertheless, a large field lies open in the immediate future for turning Baeyer's discovery to practical account. It is well

known that a great loss of coloring matter occurs in all the processes now in use for either dyeing or printing with indigo. It has already been stated that a large percentage of indigo is lost in the "cold vats" in the sediment. Another portion is washed off and wasted after the numerous dippings; whilst, in order to produce a pattern, much indigo must be destroyed before it has entered into the fibre of the cloth. Moreover, the back of the piece is uselessly loaded with color. In the processes of printing with indigo the losses are as great or even greater and, in addition, such considerable difficulties are met with that only a few firms (Potter, Grafton in Manchester and Schlieper in Elberfeld) have been successful in this process. But a still more important fact remains, that no printing process exists in which indigo can be in combination with other colors in the ordinary way, or without requiring some special mode of fixing after printing. Hence it is clear that the weak points of natural indigo lie in the absence of any good process for utilizing the whole of its coloring matter and in the impossibility, or, at any rate, great difficulty, of employing it in the ordinary madder styles of calico printing. Such were the reasons which induced the patentees to believe that, although the artificial dye cannot be made at a price to compete with natural indigo for use in the ordinary dye-beck, it can even now be very largely used for styles to which the ordinary dyestuff is inapplicable.

To begin with, Baeyer employed (Patent 1177) grape-sugar as a reducing agent. The reduction in this case does not take place in the cold, and even on long standing only small traces of indigo are formed, but if heated to 70° or upwards the change takes place. Unfortunately this production of indigo-blue is rapidly followed by its reduction to indigo-white and it is somewhat difficult in practice to stop the reaction at the right moment. But "necessity is the mother of invention," and Dr. Caro, of Mannheim, to whom the speaker is greatly indebted for much of the above information, found that sodium xanthate is free from many of the objections inherent to the glucose reduction process, inasmuch as the reaction then goes on in the cold. Moreover, he finds that the red isomeride of indigo-blue, indirubin, which possesses a splendid red color, also occurring in natural indigo, but whose tinctorial power is less than that of the blue, is produced in less quantity in this case than when glucose is employed. On this cloth, alumina and iron mordants may be printed and this afterwards dyed in alizarin, etc., or this coloring matter may also be printed on

the cloth and the color fixed by moderate steaming without damage to the indigo-blue. This process is now in actual use by printers both in England and on the Continent; so that, thanks especially to the talent and energy of Dr. Caro, Baeyer's discovery has been practically applied within the short space of twelve months of its conception. Operations on a manufacturing scale have been successfully carried on in the Baden Soda and Aniline Works at Ludwigshafen for the last two months and the directors see no reason why they should not be able to supply any demand, however great, which may be made for ortho-nitro-phenyl-propiolic acid.

The proper way of looking at this question at present is, therefore, to consider ortho-nitro-phenyl-propiolic acid and indigo as two distinct products not comparable with each other, inasmuch as the one can be put to uses for which the other is unfitted and there is surely scope enough for both. Still, looking at the improvements which will every day be made in the manufacturing details, he must be a bold man who would assert the impossibility of competition with indigo in all its applications. For we must remember that we are only at the beginning of these researches in the indigo field. Baeyer and other workers will not stay their hands, and possibly other coloring matters of equal intensity and of equal stability to indigo may be obtained from other as yet unknown or unrecognized sources, and it is not improbable that these may turn out to be more formidable competitors in the race with natural indigo than ortho-nitro-phenyl-propiolic acid.

Looking at this question of the possible competition of artificial with the natural indigo from another point of view, it must, on the other hand be borne in mind that the present mode of manufacturing indigo from the plant is extremely rude and imperfect, and that by an improved and more careful carrying out of the process great saving in coloring matter may be effected, so that it may prove possible to produce a purer article at a lower price and thus to counterbalance the production of the artificial material.

The following are the directions issued by the patentees to calico printers for using the new color:

Printing with Artificial Indigo.

No. 1.—*On Unprepared Cloth.*

Standard.—Take 4 lbs. propiolic acid paste (equal to 1 lb. dry acid)

and 1 lb. borax, finely powdered; mix well. The mixture first becomes fluid, and at last turns stiff. Then add 3 quarts white starch thickening (wheat starch), mix well and strain.

Printing Color.—Take the above standard and dissolve in it immediately before printing $1\frac{1}{2}$ lbs. xanthate of soda, stir well, and ready for use.

For lighter shades, reduce the above printing color with the following: In 1 gallon white starch paste dissolve 1 lb. xanthate of soda.

Directions for Use.—Print and dry as usual. The pieces ought not to be placed in immediate contact with drying cylinders, or otherwise be subjected to heat above 100° C. The indigo-blue is best developed by allowing the printed goods to remain in a dry atmosphere and at an ordinary temperature for about 48 hours. Damp air ought to be excluded as much as possible until the color is fully developed; then the pieces may be passed through the ageing machine, or steamed at low pressure if such treatment should be required for fixing any other color or mordant printed along with the indigo-blue.

After the blue is ready formed the pieces are first thoroughly washed in the washing-machine, and *then boiled* in the clean water, or better, in a weak solution of hyposulphite of soda (1 lb. to 10 gallons), *and at a full boil* for half an hour, in order to volatilize the smell, which would otherwise adhere to the goods.

Clean in a soap-bath at a temperature not above 40° C.; wash, dry and finish.

Observations.—Wheat starch gives the best results in the color, then follows gum tragacanth. The color is considerably reduced by using gum senegal, dark British gum or calcined farina as thickening materials.

So far borax has answered best as an alkaline solvent of propiolic acid; it may, however, be replaced in the above standard by acetate of soda (from 1 to $1\frac{1}{2}$ lbs.) or by 6 ozs. pearlash or soda. Any excess of caustic potash or soda destroys propiolic acid.

The above standard keeps unchanged for any length of time. It is likewise not sensibly altered by a small amount of xanthate of soda, but when mixed with its full proportion of xanthate, as in the above printing color, it gradually loses strength after several hours.

The xanthate ought, therefore, to be mixed with the standard immediately before printing, and any color remaining unused may then be saved by mixing with the same a large proportion of starch paste.

Propiolic acid may be printed along with aniline-black, catechu brown and drabs, and with alumina and iron mordants for madder colors.

After the indigo-blue is fully developed, the mordants are fixed in the ordinary manner, dyed with alizarin, padded with Turkey-red oil, steamed, and otherwise treated as usual.

Indigo-blue, whether natural or artificial, suffers by prolonged steaming at high pressure. For this reason only such steam colors can be associated with propiolic acid as may be fixed by short steaming at low pressure.

No, 2.—*On Prepared Cloth (for Full Shades.)*

Dissolve 2 lbs. of xanthate of soda in 1 gallon of cold water. Pad the goods with the above, dry, print with standard, and after printing follow the above treatment. The pieces may also be first printed with xanthate and then covered with standard. Alumina and iron mordants for madder colors may be likewise printed on cloth thus prepared, or printed with xanthate of soda.

The potential importance, from a purely commercial point of view, of the manufacture may be judged of by reference to the following statistics, showing that the annual value of the world's growth of indigo is no less than four millions sterling.

Estimated Yearly Average of the Production of Indigo in the World, taken from a Total Crop for a Period of Ten Years.

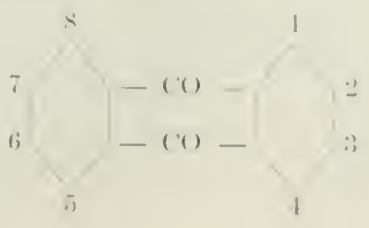
	Pounds weight.	Pounds ster.
Bengal, Tirhoot, Benares and N. W. India,	8,000,000	2,000,000
Madras and Kurpah, . . .	2,200,000	400,000
Manilla, Java, Bombay, etc., . .	....	500,000
Central America, . . .	2,250,000	600,000
China and elsewhere consumed in the country,	. ...	say 500,000
		4,000,000

How far the artificial will drive out the natural coloring matter from the market cannot, as has been said, be foreseen. It is interesting, as the only instance of the kind on record, to cast a glance at the history of the production of the first of the artificial vegetable coloring matters—alizarin. In this case the increase in the quantity produced since its discovery in 1869 has been enormous; such, indeed, that the artificial color has now entirely superseded the natural one to the almost complete annihilation of the growth of madder root. It appears that

whilst, for the ten years immediately preceding 1869, the average value of the annual imports of madder root was over 1,000,000 sterling, the imports of the same material during last year (1880) amounted only to £24,000. The whole difference being made up by the introduction of artificial alizarin. In 1868 no less a quantity than 60,000 tons of madder root were sent into the market, this containing 600,000 kilos. of pure natural alizarin. But, in ten years later, a quantity of artificial alizarin, more than equal to the above amount, was sent out from the various chemical factories; so that in ten years the artificial production had overtaken the natural growth, and the 300,000 or 400,000 acres of land, which had hitherto been used for the growth of madder, can henceforward be better employed in growing corn or other articles of food. According to returns, for which the speaker had to thank Mr. Perkin, the estimated growth of madder in the world previous to 1869 was 90,000 tons, of the average value of £45 per ton, representing a total of £4,050,000.

Last year (1880) the estimated production of the artificial coloring matter was 14,000 tons, but this contains only 10 per cent. of pure alizarin. Reckoning one ton of the artificial coloring matter as equal to 9 tons of madder, the whole artificial product is equivalent to 126,000 tons of madder. The present value of these 14,000 tons of alizarin paste, at £122 per ton, is £1,568,000; that of 126,000 tons of madder, at £45, is £5,670,000, or a saving is effected by the use of alizarin of considerably over 4,000,000 sterling. In other words, we get our alizarin dyeing done now for less than one-third of the price which we had to pay to have it done with madder.

Our knowledge concerning the chemistry of alizarin has also proportionately increased since the above date. For whilst at that time only one distinct body having the above composition was known, we are now acquainted with no less than nine of the ten dioxy-anthraquinones whose existence is theoretically possible, according as the positions of the two semi-molecules of hydroxyl are changed.



Of the nine known dioxy-anthraquinones only one, viz., alizarin, or that in which the hydroxyls are contained in the position 1, 2, is actually used as a coloring agent. Then again, three trioxy-anthraquinones, $C_4H_5O_2(OH)_3$, are known. One of these is contained in madder root, and has long been known as purpurin. The other trioxy-anthraquinones can be artificially prepared. One, termed anthrapurpurin, is an important coloring matter, especially valuable to Turkey-red dyers as giving a full or fiery red. The other, called flavo-purpurin, gives an orange dye with alumina mordants. All these various coloring matters can now be artificially produced, and by mixing these in varying proportions a far greater variety of tints can be obtained than was possible with madder alone, and thus the power of diversifying the color at will is placed in the hands of the dyer and calico printer.

It is quite possible that in an analogous way a variety of shades of blue may be ultimately obtained from substituted indigoes, and thus our catalogue of coal-tar colors may be still further increased.

To Englishmen it is a somewhat mortifying reflection that whilst the raw materials from which all these coal-tar colors are made are produced in our country, the finished and valuable colors are nearly all manufactured in Germany. The crude and inexpensive materials are, therefore, exported by us abroad to be converted into colors having many hundred times the value, and these expensive colors have again to be bought by English dyers and calico printers for use in our staple industries. The total annual value of manufactured coal-tar colors amounts to about three and a half millions; and as England herself, though furnishing all the raw material, makes only a small fraction of this quantity, but uses a large fraction, it is clear that she loses the profit on the manufacture. The causes of this fact which we must acknowledge, viz., that Germany has driven England out of the field in this important branch of chemical manufacture, are probably various. In the first place, there is no doubt that much of the German success is due to the long-continued attention which their numerous universities have paid to the cultivation of organic chemistry as a pure science, for this is carried out with a degree of completeness and to an extent to which we in England are as yet strangers. Secondly, much again is to be attributed to the far more general recognition amongst German than amongst English men of business of the value, from a merely mercantile point of view, of high scientific train-

ing. In proof of this it may be mentioned that each of two of the largest German color works employs no less a number than from 25 to 30 highly educated scientific chemists, at salaries varying from £250 to £500 or £600 per annum. A third cause, which doubtless exerts a great influence in this matter, is the English law of patents. This, in the special case of coloring matters at least, offers no protection to English patentees against foreign infringement, for when these colors are once on the goods they cannot be identified. Foreign infringers can thus lower the price so that only the patentee, if skillful, can compete against them, and no English licencees of the patent can exist. This may, to some extent, account for the reluctance which English capitalists feel in embarking in the manufacture of artificial coloring matters. That England possesses, both in the scientific and in the practical direction, ability equal to the occasion, none can doubt. But be that as it may, the whole honor of the discovery of artificial indigo belongs to Germany and to the distinguished chemist Professor Adolf Baeyer, whilst towards the solution of the difficult problem of its economic manufacture, the first successful steps have been taken by Dr. Caro and the Baden Aniline and Soda Works of Mannheim.

Influence of Varying Pressure upon Pendulums.—M. Saint-Loup finds, as a first result of his experiments upon the influence of atmospheric pressure upon the duration of pendulum oscillations, that there is an increase of about $\frac{1}{10}$ of a second per day for a fall of ten millimetres in the barometer. He does not attach much importance to this figure, regarding it merely as an indication of the order of magnitude of the disturbances; but it seems to show the importance of a correction for pressure in all calculations of exact time. Tresca states that when the conference was held, under the direction of Le Verrier, for the construction of three Parisian regulators of precision, one of the constructors, M. Redier, had fitted to the pendulum a metallic barometer, with an arm which was displaced so as to compensate the variations of retarding influence in the atmospheric pressure.—*Comptes Rendus.* C.

UNIVERSAL ENERGY OF LIGHT.*

By Pliny Earle Chase, LL.D.
Professor of Philosophy in Haverford College.

Force is generally regarded as a function of mass and velocity. The greatest known velocities which can be produced by central forces are wave velocities. The greatest known wave velocity which appears to be universally diffused is the velocity of light.

Let v_λ = velocity of light; v_o = circular orbital velocity at Sun's surface $= \sqrt{g_o r_0}$; v_3 = Earth's mean orbital velocity; v_r = velocity of Sun's equatorial rotation; u_3 = potential velocity of water at 0°C. $= \sqrt{2g \times 100 \times 1389{\cdot}6 \text{ ft.}}$; u_4 = potential velocity of water at its maximum density; u_s = potential velocity of water evaporation $= \sqrt{2g \times 536{\cdot}37 \times 1389{\cdot}6 \text{ ft.}}$; m_0, m_3, m_5, m_6 = masses of Sun, Earth, Jupiter, Saturn; h_o = Earth's semiaxis major; h_2 = height of mean oscillatory projection due to the combining energy of H_2O; t_a = time of acquiring circular orbital velocity at Laplace's limit of synchronous rotation and revolution = time of rotation $\div 2\pi$; t_n = time of acquiring "nascent" or dissociative velocity at nucleal surface $= \frac{1}{2}$ time of rotation $= \pi t_a$; $\varkappa$ = Weber's electro-chemical unit; μ = electro-magnetic unit; φ_o = total magnetic force; φ_3 = terrestrial magnetic force; t_o = present value of t_n at Sun's surface; g_o = gravitating acceleration at Sun's surface.

The simplicity of the relations of the universal velocity (v_λ) to other physical velocities is shown in the following equations:

$$1. \quad \frac{v_\lambda}{u_3} = \frac{h_o}{h_2} = \frac{m_o}{m_3} = \left(\frac{t_n}{t_a}\right)^2 . \sqrt{\frac{\varphi_o}{\varphi^3}}$$

$$2. \quad \frac{v_\lambda}{v_o} = \frac{v_o}{u_4} . \sqrt{2} = \frac{t_n}{t_a} . \frac{v_o}{v_r}$$

$$3. \quad \frac{v_\lambda}{g_o} = t_o$$

* Abstract of a paper read before the American Association for the Advancement of Science, August, 1881.

$$4. \quad \frac{r_\lambda}{r_3} = \frac{\sqrt{m_{\circ} m_3}}{m_3} = \frac{m_{\circ}}{m_3} \frac{t_{a}}{t_{n}} \cdot \sqrt{\frac{\mu}{\varkappa}}$$

$$5. \quad \frac{r_\lambda}{u_{a}} = \frac{3^4 m_{\circ}}{2\ m_6} = \frac{5 \times 3^3 m}{m_5}$$

The velocity of solar atmospheric rotation, at the secular mean centre of gravity of the solar system, is also equivalent to u.

The law of conservation of areas, in an expanding or contracting nucleus, requires that $g_{\circ}$ should vary inversely as t. Equation 3 should, therefore, hold good for all stages of solar existence, past, present and future.

The values which satisfy the above equations are: $m_{\circ} = 328470\ m_3$; $h_{\circ} = 92476500$ miles; $r_\lambda = 185760$ miles; $r_3 = 18{\cdot}412$ miles; $u_3 = 2986$ ft.; $u_{\circ} = 6916{\cdot}2$ ft.

The following table shows the accordance between theoretical and observed values.

	Theoretical.	Observed
Boiling point of water, .	99·18°	100°
Combining heat of H_2O., .	69319	67616 to 69584*
$\varsigma_{\circ}$, . . .	140·65	140 lb. per sq. in.
Maximum density of water,	4·19°	3·33° to 4·85°
r_3, . . .	18·31	18·41
Latent heat of steam, .	536·374°	536·385 †
$\varkappa \div \mu$, . .	107·38	106·67

The velocity of light is also a factor of electromotive energy. Weber and Kohlrausch demonstrated this fact by measuring quantity of electricity; Thomson and Maxwell by measuring electromotive force; Ayrton and Perry by measuring electrostatic capacity.

Perhaps the most interesting of the above indications is the past, present and future equivalence of Sun's "nascent" velocity to the velocity of light, the sum of the cyclical reactions of solar superficial gravitation against the actions of external gravitation, during each half-rotation, being *equivalent to the velocity of light.*

* The mean of six estimates, cited by Neumann, is 68886.

† This is the mean of four estimates, viz.: Favre and Silbermann, 535·77; Andrews, 535·90; Regnault, 536·67; Tyndall, 537·20.

Algerian Wine.—The portions of Algeria which are near the shores of the Mediterranean are very favorable to the growth of grapes. The colonists are rapidly removing the dwarf palm trees and the underbrush, in order to plant vineyards. The change is going on with wonderful rapidity. The vines bear fruit after three years, and they are at present subject to no injury except from the sirocco and white frost in some localities. The income after five or six years reaches a mean value of 4000 francs per hectare (324 dollars per acre). —*Les Mondes.* C.

Disturbances of Telephonic Transmission.—M. A. Gaiffe has noticed electrical currents in bars of iron which are set in vibration by shocks. He attributes them to induction. According to Ampère's theory he thinks that a vibrating magnet should produce currents analogous to the extra currents which would arise in a vibrating solenoid.—*Les Mondes.*

[Mousson studied the influence of vibrations on the conductibility of wires in 1858. In 1864 Chase showed that the daily and annual variations of the magnetic needle may be imitated by exposing them to mechanical vibrations similar to those which are produced by thermal conditions. C.]

Compensation of Geodetic Triangles.—E. Adan discusses the triangulation of Belgium, in which the network is so united as to furnish satisfactory bases for calculations relative to the dimensions of the globe. Two bases were measured nearly on the same parallel, one at Lommel, the other near Ostende. They were united by thirty-nine triangles having the Antwerp coast near the middle. Sixteen triangles separate this coast from the Lommel base and twenty-three from that of Ostende. The probable error of the combined measurements was less than $\frac{1}{4400}$. This approximation would seem sufficiently close, but by means of intermediate compensations the probable error was reduced to less than $\frac{1}{27800}$ of one per cent. It is rare that in calculations of so complicated a character a comparison can be readily made between two coordinate methods, and the result in this case is such as to recommend the system of compensations which was undertaken. —*Bull. de l'Acad. Belg.* C.

Felling Trees with Dynamite.—M. Pissot, the forester of the Bois de Boulogne, has experimented with dynamite upon several trees which it was thought desirable to remove. He finds that dynamite can be used very advantageously, both in point of economy and rapidity of execution, for uprooting and dividing stumps of trees, but it is not applicable to felling trees which are to be used as timber.—*Les Mondes.* C.

Improvement in Compasses.—Miller and Pfaundler have devised a method for reading the indications of the compass with great accuracy. The magnetic needle carries near its ends two thin discs of aluminum; upon each of the discs a fine line is drawn so as to indicate the plane in which the pivot of the needle stands. A microscope and vernier are attached, together with a small disc of paper or mica, which stands perpendicularly upon the needle so as to bring it more speedily to rest by means of the resistance of the air.—*Dingler's Journal.* C.

Absolute Measure of Currents by Electrolysis.—After having introduced a system of absolute measures for electric magnitudes, Weber determined the electro-chemical equivalent of water, or the weight of water decomposed in a second by a unit of electromagnetic intensity. His valuation of the equivalent was ·009376 of a milligramme. The experiment has been repeated by various observers, but the results present variations of about 2 per cent. Mascart has repeated the investigation with numerous new precautions to eliminate possible causes of error, and he finds the equivalent ·009373 of a milligramme.—*Comptes Rendus.* C.

Influence of Sun Spots upon Temperature.—J. Liznar has compared the observations at St. Petersburg, Caterinenburg, Barnaul, Prague, Brünn, Vienna, Kremsmünster, Trieste, Rome, Calcutta, Batavia and Hobarttown, and finds marked evidence of a relation between the oscillations of daily temperature and of sun spots. The minima of daily oscillation correspond very closely with the maxima of sun spots; the maximum of oscillation precedes the minimum of sun spots by about two years. In the annual temperature the maximum of spots corresponds to the maximum of oscillation and the minimum of spots to the minimum of oscillation.—*Les Mondes.* C.

Upper Atmospheric Currents.—Count D'Epiennes has conducted meteorological researches which were based only upon observation, without bias by any theoretical ideas, and which lead him to the following conclusions: 1. The air rises from zones of low pressure towards the upper regions and then flows towards the centres of high pressure. 2. As the current proceeds in the upper regions it tends to form a depression. The culmination and deviations which tempests undergo in their march are governed by upper currents directed towards their centre.—*Ciel et Terre.* C.

New Heating Apparatus.—The great calorific capacity of water has hitherto rendered it the most usual reservoir of utilized heat. There are, however, some fusible bodies which allow the storing of a much greater quantity of heat in the same volume without increasing the temperature of the vessels which contain them. Acetate of soda, for example, when melted, contains about four times as much available heat as water. M. A. Ancelin has accordingly experimented with it in suitable boxes for warming cars and carriages. His experiments upon the French railroads have been so satisfactory that his system seems likely to be adopted by many of the roads in Portugal, Italy, Spain and England.—*Comptes Rendus.* C.

Franklin Institute.

HALL OF THE INSTITUTE, Sept. 21st, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 38 members and 3 visitors.

The minutes of the last meeting were read and approved.

Mr. Tatham then said: "Before proceeding to any business, I feel it proper to bring before the meeting the mournful circumstances under which we are assembled. The President of the United States has just died after a long struggle against the death wound received some months ago. I feel there is not a citizen who participated in the election which made General Garfield President who does not feel his loss as a personal sorrow. I feel sure we all regard it as a public calamity. The opening months of his administration were so full of

fruit that we all indulged in the anticipation of a bright future, and whatever may be the course of events hereafter, the picture of his bright administration will be without a shadow. He has left us a great example. To the young, an example of what may be accomplished by diligence, study and a well-directed ambition; to men, an example of the power of justice, moderation and candor; and to all an example of cool courage in the face of death, and of patient resignation to the will of the Almighty."

Mr. Samuel Sartain moved that, in view of our great national bereavement, the Institute now adjourn, which was carried.

ISAAC NORRIS, M.D., *Secretary.*

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Errata to article on "Properties of Air relating to Ventilation and Heating," Vol. CXII, August, 1881. Correct one column of Table, page 145, to read as follows:

	Rates of exhalations to inhalations. Vols.
Nitrogen,	1·0381
Oxygen,	0·8321
Aqueous vapor,	2·9787
Carbonic acid,	102·73
Total,	1·0703

Also remove the words per cent. from three places where they occur in the Table.

Again, on page 143, place 1 as the numerator of the fraction $\frac{1}{1000000}$.

// JOURNAL

OF THE

FRANKLIN INSTITUTE.

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

Vol. CXII. NOVEMBER, 1881. No. 5.

AN IMPROVED DYNAMOMETER.

By William P. Tatham.

The mechanical world has long desired a correct and convenient dynamometer, capable of general application. All of the instruments heretofore presented fail in some of the desired qualities. The Proney Brake, which is the most correct, absorbs the power, and does not transmit it. Dynamometers of the Morin type, which are next in accuracy, are of limited application to small powers, and of the many others, it would be hard to find one in which the friction of the instrument in not largely represented in its indications.

To the old requirements for a good dynamometer we have now superadded the demand growing out of the generation of electricity from mechanical power, and accuracy is here, as ever, the most to be desired. Under these circumstances, which sufficiently attest the difficulties of the case, I venture to describe an instrument involving the application of a new principle, in the hope that it may satisfy a pressing want.

Referring to Fig. 1, the arrows show the movement of the belts; the frame of the machine is omitted, the more clearly to exhibit the working parts. Let A represent the first motion pulley of the dynamometer, upon a shaft receiving power from any source outside; B B′

two semicircular vibrating frames, having freedom to move around their central knife edges, C, C′, which play in valleys formed by the intersection of two small planes. These frames are linked together by two links at D and D′, also on knife edges. The frame B carries two pulleys, E and F. The pulley E is adjusted and centred upon the frame so that the central knife edge C coincides with the point of tangency of the belt (a) with the pulley E, the tangent point being taken at the middle of the thickness of the belt. By this arrangement the force of the belt passes directly through the fulcrum of the frame B, and therefore exerts no influence to vibrate it.

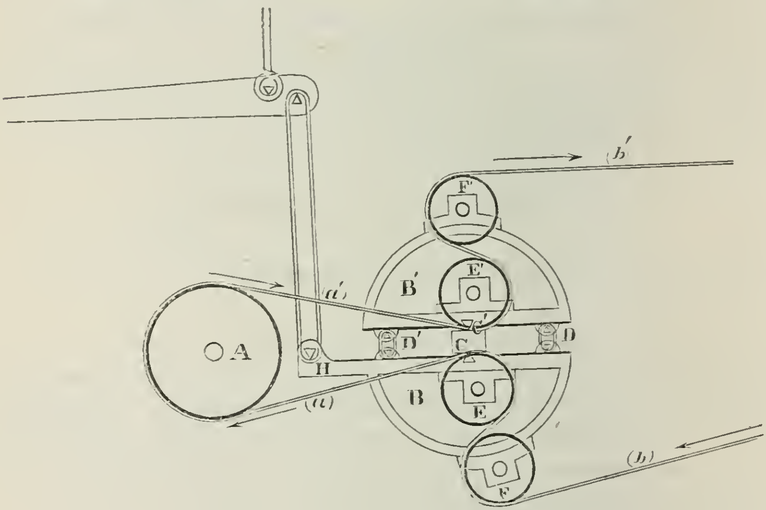


Fig. 1.

The pulley F is adjustable around the circumference of the frame B, and is placed in such a position that the belt (b), passing from the machine on trial, makes at its point of tangency with the pulley F a right angle with a straight line joining the tangent point to the central knife edge C. The frame B has an arm attached to it, to carry the knife edge H, to which the link of the scale beam is attached. The distance of this knife edge H from knife edge C is equal to the distance of the latter from the point of tangency of pulley F and belt (b), taken as before at the middle of the thickness of the belt.

The result of this disposition is that the only influence to cause a vibration of the frame B is the reaction of the belt (b), which is exactly equal to the action of the same belt upon the machine on trial. In

the same manner the action of the slack belt (b′) upon the frame B′ is exactly equal to its reaction upon the machine on trial. The frames B and B′ being connected together by the links and knife edges, the difference of the tensions on (b) and (b′) is exerted to vibrate the two frames, and this difference alone is felt by the scale beam, all friction being eliminated except the friction of the knife edges. The rudely constructed working model which has been tried indicates with surprising accuracy when tested by a brake. To prove the elimination of the friction, the wheels E′ and F′ where chocked without change of indicated power.

I see no reason to doubt that this machine, when properly conducted and adjusted, would indicate correctly the power absorbed by the train of a watch, or given out by a water wheel; but it is still lacking in one very essential quality of a good machine—*it would not work well when out of order.*

When the journals of the wheels on the vibrating frames become worn by long use these wheels will be displaced, and their points of tangency with the belts will be displaced also, and the accuracy of the indications will be impaired.

In order to meet this requirement I have embodied my ideas in a different form, represented in Fig. 2. The arrows, as before, show the direction of the belts. A is the first motion pulley and shaft; a and a′ the tight and slack belts to the pulleys carried on the vibrating frame B. These belts do not pass through the point of tangency, but *their direction does.* The vibrating frame B is balanced upon the knife edges C, and is provided with knife edges H, which engage the links of the scale beam.

The distance from C to H is equal to the effective diameter of the pulleys E, E′ upon the vibrating frame; b and b′ are the tight and slack belts from these pulleys to the pulley M, which now takes the place of the machine on trial in the description of the machine represented in Fig. 1. It is important that the belts b b′ should not make a less angle with the vertical line than the belts a a′.

The index will show the force exerted on the pulley M, whose friction will be included. The friction of the pulley and shaft M can be estimated accurately by providing simple means for weighing the combined tensions of the belts b and b′ upon the pulley M, and then, with the indications furnished by the dynamometer when running light and loaded, the friction when loaded can be calculated.

The speed of the belt may be measured by the counter, and the whole indication of power may be ascertained by combining a multiplied movement of the vibrating frame with a regular movement of a paper band fed from one of the shafts.

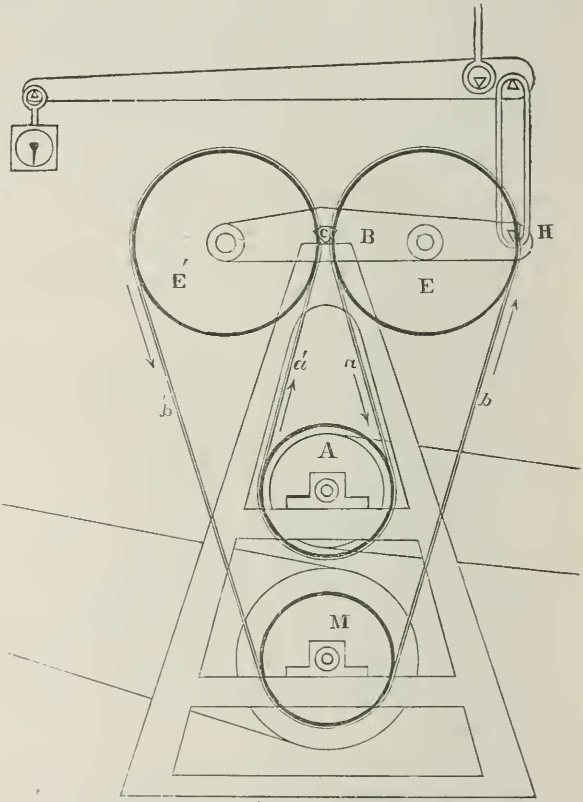


Fig. 2.

A critical examination of the last-described machine will show that no probable wear and no probable change of the thickness of the belt can interfere materially with the accuracy of its indications.

The vibration of the frames need be very small. In the working model (if it were strong enough to bear it) 320 lbs. would be indicated by the spring balance on the scale beam, and would correspond to a movement of ·0375 inch of the knife edge H.

In estimating the middle of the thickness of the belts as the points from which to measure the effective diameters of the pulleys governing

the feed of belt and transmission of force I have followed the books which have been confirmed by my preliminary experiments. In constructing an exact machine this point should be carefully examined.

A third form of the dynamometer, combining the accuracy of No. 1 with the wearing properties of No. 2, may be constructed by dividing the vibrating frame B of No. 2 into two frames, connected together by links, each frame having its own fulcrum C in the line of the middle of the thickness of the belts a a′, all the belts to be made vertical and pull at right angles to the levers.

CHEMICAL METHODS FOR ANALYZING RAIL-STEEL.

By Magnus Troilius, Chemist to C. P. Sandberg, London, Eng.

Read before tha American Institute of Mining Engineers, October, 1881.

Introduction.—By C. P. Sandberg.

Since the discussion on steel rails in America has forcibly drawn attention to the value of chemical analysis, if not as a necessary stipulation, at least as a guide to control the usual mechanical tests, some doubt has been thrown upon the accuracy of the analytical results obtained by different chemists.

To any one having the least acquaintance with chemistry it is quite clear that if exactly similar results are to be obtained from the same borings of steel, exactly the same methods must be used by the different analysts. Hence the necessity (if complications are to be avoided) of establishing what I may call standard or normal methods, to be used both by the inspectors and by the chemists at the works. Remembering that the application of chemistry to steel rail inspection is yet in its infancy, it is of great importance to possess a perfect acquaintance with the best methods in use.

Being myself a grateful pupil of Professor Eggertz, of the School of Mines in Sweden, it occurred to me, two years ago, that I could not do better than start a laboratory of my own, and engaged one of his pupils, Mr. Troilius, for the purpose of analyzing the steel borings from mechanically-tested rails, so that I might thus obtain, without delay, thoroughly accurate determinations.

Moreover, in order to carry on the operations in perfect accordance with the methods used at the steel-works in England and Germany, where I had to control the manufacture, I deemed it desirable to allow

Mr. Troilius to go through a course of training at these works; and I gladly seize this opportunity of expressing my grateful acknowledgments to several works in England and in Germany for affording every facility for such an exchange of information as was found necessary to arrive at the best analytical methods to be used.

The value of this preliminary training has already become evident, for, after working a year in my own laboratory, we find that the results hardly ever differ from those obtained at the works; or, at any rate, they rarely differ from the results of those professional chemists who check the same borings in case of any discrepancy.

Inasmuch as this work is principally executed for America, it naturally follows that if it is checked by American chemists it will be of interest, both for them and for the chemists on this side of the Atlantic, to know the methods of analysis followed in the two countries. With this view, I beg to introduce the following paper, which has been very carefully worked out by Mr. M. Troilius, and therefore deserves the attention of the members of the Institute.

I can only say of this paper, as I have said in my own, "On the Specification and Inspection of Steel Rails in Europe," that it fully explains the methods, which I have hitherto adopted, with excellent results. But if any better methods can be suggested by American chemists, I shall only be too glad to modify my present mode of working.

It only remains for me to add that Mr. Troilius will have great pleasure in answering any remarks or questions that may be addressed to him with reference to the following methods for the application of chemistry to steel rail inspection.

19 Great George Street, Westminster, London, August, 1881.

Chemical Methods for Analyzing Rail Steel.

Useful Appliances.—One of the most useful and necessary appliances in a steel laboratory is the hot-plate. An iron plate, 12″ x 18″ x $\frac{5}{8}$″ thick, heated from below by a good Bunsen burner, will answer very well; or, if more convenient, the plate may be combined with a coke fire and a muffle-furnace, the coke fire thus heating both the plate and the muffle-furnace. In any case, the plate should be heated in such a manner as to have a boiling temperature at only one part, from which part the heat should gradually decrease towards the edges. Thus arranged, the plate forms a very satisfactory sub-

stitute for water-baths, drying-boxes, etc., as will be seen in the following description of the methods. There is no risk of destroying beakers, etc., after sufficient experience in working the plate has been acquired.

A plate of this kind has been in use in the London School of Mines for about fifteen years, but, as far as I am informed, it was Mr. Snelus who first employed a plate in this way at Dowlais, and it has since become universally used in English and Welsh laboratories. I should also mention that it has been successfully introduced in the Stockholm School of Mines, in accordance with my suggestion last year in the *Jernkontorets Annaler*. In German steel laboratories the water-bath and other more "scientific" appliances are more generally in use than in England and Wales; in fact, I have not so far seen the plate used in any of the German steel works which I have visited. It is necessary to have the plate placed under a good draught, so as to remove all the noxious fumes which are evolved during the operations conducted on the plate.

As regards other useful appliances belonging to a well-fitted steel laboratory, they are all more or less common or are only occasionally used, and do not deserve as much attention as the above-named plate. Fluted funnels, however, are worthy of being mentioned as being somewhat quicker to work with than ordinary plain ones. I have not found them in use on the continent as frequently as in the United Kingdom, but they are now beginning to be somewhat more appreciated, even in the continental countries.

Carbon Determination.—Eggertz's color-test is a very accurate method for determining carbon in rail-steel, provided the operator has sufficient experience and takes all the necessary precautions. At the same time, this method has the great advantage of being very rapid. It is now nearly twenty years since this method was described in Sweden and Germany by Professor Eggertz, and very shortly afterwards Mr. C. P. Sandberg published an English translation of the method in the *Chemical News*. In Great Britain the color-test is now very largely used, every blow in the Bessemer converter being thus tested for carbon, and in the hands of skilled manipulators it gives every satisfaction. In German steel laboratories the color-test is not so much used as in Great Britain, and costly arrangements are often employed for carrying out determinations of carbon by the combustion process on a large scale. For the daily control of rail-steel, how-

ever, this is rather an impracticable arrangement, when results, accurate within 0·01 per cent., can so rapidly be obtained by the color-test.

Experience soon taught the manipulators at steel-works to modify the method in many respects so as to attain greater rapidity, thus deviating from the directions given by Professor Eggertz in 1862. There has, however, been some uncertainty as to many of the details, and as some rather serious discrepancies have occasionally occurred, especially in analyzing the harder classes of steel, there have not been wanting people denouncing the whole method. It is, therefore, with great pleasure that I am able to accompany this paper with a translation of Professor Eggertz's recent article on the subject. As the contents of this article, or at least the more important points, were kindly communicated to me some weeks before it was published in Sweden, I have had ample opportunity of applying the experience thus gained, and have found it thoroughly corroborated by my own results.

The most important facts in Professor Eggertz's paper are contained in the rules given for (1) quantity of acid required for each 0·1 gramme of steels of different percentages of carbon, and (2) minimum addition of water required for each 0·1 gramme of steel dissolved in nitric acid to remove the iron color.

Referring to the complete* translation, for further information I will now explain how I carry out my determinations of carbon in rail-steel by means of the color-test. My mode of manipulation is the same as that used at most English and Welsh steel-works, with the modifications of the two above-mentioned new rules of Professor Eggertz.

I use 0·2 gramme of the steel for testing, and along with every set of samples 0·2 gramme of standard steel is dissolved. This is indispensable with the mode of procedure I adopt, no precautions being taken to exclude the sunlight, etc. The solution is effected in test-tubes 6 inches long and about $\frac{5}{8}$ inch internal diameter. The dimensions of the test-tubes are not a matter of great inportance, but they should not be too narrow.

The nitric acid I always allow to flow into the tubes from a graduated burette, this being by far the best way of adding the acid. The tubes are then put into a beaker 4 to 5 inches high, half filled with

*The accompanying translation contains an addition made after publishing the article in Sweden, and is thus believed to be more complete than any translation which has already appeared in print. See Appendix.

water. The beaker may be advantageously covered with a perforated tin plate, and the tubes put through the holes, and thus steadied. Heat is then applied, and boiling is continued until the steels are dissolved; this seldom requires more than half an hour, and is greatly promoted by the jumping of the tubes in the boiling water. When the solution is completed the tubes are put into cold water, and the determination of carbon is thereupon carried out by means of the carbon-tubes. The carbon-tubes are generally bought in sets of three tubes, one of which is graduated and the other two not. The tubes in each set are selected carefully so as to be of the greatest possible uniformity as to dimensions and quality of glass; their capacity is 20 cc.

In my ordinary work I put the standard-steel solution into one of the ungraduated tubes, measuring off by aid of the graduated tube, and in very particular analyses I use standard-steel solutions of different colors in both the ungraduated tubes. This helps the eye to catch faint differences in tint. The solutions for testing are put into the graduated tube.

The differences in the results, which are sometimes obtained when analyzing the harder classes of steel by the color-test, are not observed as far as rails are concerned, and I find no difficulty in obtaining accurate results in this case. In fact, as far as my experience goes, the carbon in rails is that element which can be most easily accurately determined, and this by the simple color-test. But even for harder steels a very much greater certainty is now secured by the method described in Professor Eggertz's latest publication.

It is always desirable, if not necessary, that the standard-steel should have a percentage of carbon not differing too widely from the average percentage of carbon in the steels for testing; especially when dealing with *very* soft steels one finds the necessity of having a soft standard.

In working this method for carbon estimation, when the carbon ranges from 0·10 to 0·80 per cent., I have obtained accurate determinations with great rapidity; and this, indeed, is the great value of the method, which is best seen by its application for ascertaining the carbon in every blow, even at the largest steel-works in England, where hundreds of charges are made per day. Ordinarily, a boy is trained to do this work, under superintendence of the chief chemist, and consequently the cost of execution is but very small.

The plan of dissolving rapidly, and then cooling the tubes, as just described, was originally employed by Mr. Snelus at Dowlais.

Phosphorus Determination.—The greater number of steel-works which I have visited use the Eggertz's molybdic method for determination of phosphorus in steel. Except in Sweden, however, it is only at one large works in Germany that I have seen this method carried out in the way originally described by Professor Eggertz, and at those works superior appliances, etc., enable the manipulators to obtain pretty quick, and certainly very accurate, results, even when using weighed filters, and working upon the small prescribed quantity of 1 gramme. At other German works it was the practice to redissolve the phospho-molybdate obtained, and to finish the analysis by the magnesia method. But at all the English and Welsh works with which I am acquainted the phosphorus is estimated by weighing the phospho-molybdate itself. Several grammes of the steel are always used, and the precipitate is generally brushed off from the filter. Only at one Welsh works have I seen it gently burnt, so as merely to incinerate the filter.

The former of the two last-named modes of manipulation is the one I use. Like so many other useful modifications in the chemistry of iron and steel, this plan was originally introduced at Dowlais by Mr. Snelus. In the following pages I will describe the process, and, at the same time, refer briefly to the Welsh "burning" method and to the magnesia method, etc.

The Burning Method.—The solution of the steel for the determination of phosphorus is an easy operation. Not less than 5 grammes of steel are dissolved in a mixture of equal volumes of strong nitric and hydrochloric acids. (I use for this purpose nitric acid 1·42, and hydrochloric acid 1·195 sp. gr.) No loss through escape of phosphorus in combination with hydrogen is hereby incurred. The solution is evaporated to dryness, and heated until all dark fumes have ceased to escape. A beaker or a porcelain dish may be used, according to circumstances, and evaporations, etc., are performed on the hot plate. By the evaporation to dryness the complete solution of the steel is secured, all organic matter is destroyed and the silica can be separated, which is advisable if it is present in any noticeable quantity.

The dry mass is then dissolved in strong hydrochloric acid, the excess of acid removed by evaporation, hot water added and the silica filtered off. (If little or no silica is present it is, of course, unnecessary to filter it off, and the precipitation of phosphorus may then at once be proceeded with.) The filtrate is evaporated down to a small bulk, so that it is only just fluid; it is allowed to cool, and then about 4 cc.

of the strong nitric acid are added. A little rinsing water is introduced so as to make the bulk about 20 cc. The beaker is strongly shaken in the right hand, while from a pipette, which is held in the left hand, 20 cc. of the solution of molybdate of ammonia are allowed to run into the beaker in a thin stream.

The solution of molybdate is prepared by dissolving 100 grams of molybdate of ammonia in 1000 cc. of water and 100 cc. of ammonia, 0·88 sp. gr. It is of no advantage to use less strong solutions of molybdate than this, as one has then to employ a larger quantity of the same, and thus obtain a greater bulk, the work being thereby retarded.

After pouring in the solution of the molybdate a few drops of ammonia (0·88) are added, and the beaker is shaken until the precipitate of iron has disappeared. The phospho-molybdate is then completely down, and you have only to leave the beaker on the less hot part of the plate at least for 1 hour, during that time allowing it to settle, and shaking it up again repeatedly. After the last shaking the precipitate must separate distinctly, and leave a perfectly clear, supernatant solution. There is no danger of getting molybdic acid down, even if you were to boil for a moment or to use a large excess of molybdate, provided that there is a sufficiently large quantity of nitric acid present; but, if there is arsenic in the steel, this will come down along with the phosphorus, and cause too high results. The using of the ammoniacal solution of molybdate and ammonia causes a considerable elevation of temperature; hence, as will be shown below, the precipitation of arsenic.

After settling, pour the liquid on a good Swedish 4-inch filter; wash the filter with cold water containing 1 per cent. of nitric acid until it is quite white; wash the precipitate in the beaker once by decanting with ordinary water, moderately hot, and finally wash the precipitate down on the filter, and collect it at the centre with as few washings as possible with ordinary water, moderately hot. The filter should be quite white before the precipitate is washed on to it.

If the washing is conducted in this way no loss will be incurred in dissolving, neither will the fluid run through turbid. The solubility of the phospho-molybdate precipitate, at 16° C., is given by Professor Eggertz as follows:

In pure water, . . .	1 part in 10,000
In water, with 1 per cent. of nitric acid,	1 " 6,600
In hydrochloric acid, 1·12, .	1 " 550
In nitric acid, 1·2, . . .	1 " 190

If to the solvent be added a solution of molybdate of ammonia, equal to about half of its volume, the solvent action seems to be considerably lessened.

After washing, unfold the filter containing the precipitate upon another filter, and put it upon the edge of the plate to dry. The unfolded filter should be covered with a large watch-glass, so as to prevent dust from getting into it. As to the temperature for drying, this is by no means so essential a point as is often supposed, and the precipitate may be dried for hours at a temperature between 100° and 140°C. without changing its percentage of phosphorus in any noteworthy degree, as stated by Professor Eggertz, whose results in this respect are compiled in the following table:

Temperature.	Loss per cent. of weight of precipitate.
95°—100°C.	0·40
100°—120°C.	0·20
120°—140°C.	0·05
95°—140°C.	0·65

The total loss in weight is thus only 0·65 per cent. when drying at 140°C., and this has no practical influence, considering the small amount of phosphorus in the precipitate and the large quantity of steel operated upon. The precipitate also retains its yellow color at the temperature of melting lead (355°C.), but gets black at the temperature of melting zinc (400°C.)

When dry, the precipitate is shaken down into a weighed platinum, or porcelain dish, the brush not being applied until nothing more can be loosened from the filter by mere shaking. It is a convenient practice to hold the filter in the left hand, and to knock gently on this hand with the other.

Having thus given the outlines of my mode of using the molybdic method, I would add the following precautions, which are necessary for attaining accurate results:

1st. Removing excess of hydrochloric acid from the solution by evaporation.

2d. Adding the solution of molybdate in a very thin stream, shaking well.

3d. Great care in the washing and brushing off.

As for the weighing, it is advisable to dry in the vessel repeatedly, and weigh two or three times before deciding the weight finally.

In *Eggertz's original method* there is used for the determination of phosphorus in steel only 1 gramme. The solution of molybdate is prepared from 100 grammes pure molybdic acid to 422 cc. ammonia (0·95 sp. gr.), and 1250 cc. nitric acid (1·2 sp. gr.). By using such a small quantity of steel one advantage is gained, viz., that no evaporation after separation of the silica is required, the bulk being very small if the washing has been carefully performed. But, of course, brushing cannot be applied when such a small quantity of steel is taken. Weighed filters have to be used.

As to the precipitation in this case, you may manage to have the iron solution of 15 to 20 cc. volume, and add to the same at least half its volume of the above-mentioned solution of molybdate. The solution is well stirred and left at a temperature of 40° C. for 1 to 3 hours, after which the precipitate is collected on the weighed filter, dried and weighed.

In Sweden it is of great importance to keep the temperature not above 40° C., as some of the very purest Swedish irons and steels contain arsenic, which will come down as a yellow precipitate similar to the phospho-molybdate. At 70° C. this arsenio-molybdate (containing 4·11 per cent. of arsenic) comes down pretty quickly, and, on boiling, it precipitates at once. It is easy to understand how important it must be to avoid estimating the arsenic as phosphorus in, for instance, a Dannemora "Walloon" iron, where the phosphorus may be only 0·01 per cent. or less. If the right temperature be neglected the phosphorus would, perhaps, appear to be 0·02 per cent.; but this would still be serious in such pure material. In fact, it is to be feared that many mistakes on the part of consumers of Swedish steels are committed through overlooking the presence of arsenic.

It falls beyond the limits of this paper to describe Prof. Eggertz's method for separating and determining the arsenic in steel, and I have only to mention that I have not been troubled with arsenic in any of the rail-steels with which I have had to do.

The Burning Method.—As to the method of burning the precipitate above referred to, I may say that I saw it practiced two years ago at

a large works in South Wales, but not until lately have I had time to try it myself. At the works in question it was the practice to put the wet filter containing the precipitate in a platinum crucible, and to place this in the forepart of a hot muffle-furnace, so as to incinerate the filter. A certain correction, as I was told, was made for the expelled ammonia, before taking down the result. However, I have found hitherto that by adding the filter to the precipitate after determination by the brushing method, in the usual way, and putting it all into a weighed porcelain crucible, then carefully charring the filter and burning it over a gentle flame from a Bunsen burner, no very different results are obtained; indeed, in most cases I have found the weight, after burning, only so much *higher* than after brushing, as would correspond with the weight of the filter-ash, and in no case have I found it less than after brushing. It appears that 140°C. is the highest temperature that can be used when only drying of the precipitate is intended, for at higher temperatures the filter paper begins to change. But, as far as the above-named experiments show, it is quite feasible to incinerate the filter without practically altering the composition of the precipitate. A continued series of such experiments will give a list the average of which will show the accuracy obtainable by the burning method.

Magnesia Methods.—In some laboratories it is usual to redissolve the phospho-molybdate obtained, and then to precipitate with magnesia mixture. This is a rather slow and wasteful way of procedure, and the direct (Riley's) method seems then preferable.

In the direct method one may use at least 10 grammes of steel. The hydrochloric acid solution is reduced by means of sulphite of soda, the excess of sulphurous acid is removed by boiling, the solution is then neutralized with ammonia and bromine added, so as to oxidize about 0·3 to 0·4 gramme of iron. The sesquioxide of iron is precipitated by means of acetate of soda, and the whole of the phosphorus in the steel is supposed to be precipitated along with it.

The precipitate is filtered off and dissolved in hydrochloric acid, about 13 grammes of citric acid are added, and the solution is neutralized with ammonia; 20 to 30 drops of magnesia mixture are then added, and some ammonia. The whole should then be left for two days, and be stirred up now and then during that time before it is finally filtered. The precipitate is washed with ammoniacal water, ignited and weighed.

It is difficult to see where the advantage of using this method would lie, as compared with the direct weighing of the phospho-molybdate. The ignited phosphate of magnesia contains 27·95 per cent. of phosphorus, and an error in weighing must have, therefore, a serious influence on the result, unless a very large quantity of the steel be used. But altogether, the magnesia method requires much more time, skill and labor than the molybdic method. To this should be added the statement by Professor Eggertz, that it is exceedingly difficult to obtain the reagents used in the magnesia method free from phosphorus, whereas in the molybdic acid method it is only the nitric acid which may contain phosphorus. Too high results are, therefore, frequently obtained by the magnesia method.

In conclusion, I may say that the above-described brushing method, which I use daily, has given very accurate results, as the same borings have been checked by some of the leading chemists of the day. As to the time required, if several analyses are performed at the same time, and suitable arrangements made, two or three, or even more, results may be obtained in a day.

Silicon Determination.—For determining silicon in rail steel I use the aqua-regia and the sulphuric acid methods. The former has been sufficiently described in connection with the phosphorus determination, and I will here, therefore, only mention the principal details of the latter.

For each gramme of steel I use 14 cc. of a mixture of sulphuric acid and water, in the proportion of 1 of sulphuric acid to 6 of water. If I wish to estimate the silicon only, no oxidizing of the solution is necessary, and I have only to boil (with exclusion of the air as far as possible) until all is dissolved, and then completely evaporate the water so as to render the silica insoluble. The white salt is then taken up with hot water and a few drops of strong hydrochloric acid, and the silica filtered off and washed with hot water containing 5 per cent. of nitric acid.

If manganese is to be estimated in the solution obtained, the solution should be boiled with a few cubic centimetres of nitric acid for about one-quarter of an hour before evaporating down. After dissolving the salt in water and hydrochloric acid, boiling should be continued for another quarter of an hour before filtering off the silica, so as to insure the manganese being converted to manganous oxide. The silica must, in this case, be washed, first with ordinary cold water, and

then with the nitric acid water, which should flow into a separate beaker and not into the first filtrate, where it might produce a higher state of oxidation of the manganese.

I find that the aqua-regia and the sulphuric acid methods yield results which are quite uniform and concordant. The sulphuric acid method can be quite as rapidly used as the aqua-regia method by means of the hot-plate. Two years ago I worked this method in a steel laboratory in Wales, and the chemists at the place took such a liking to it on account of the absence of the disagreeable fumes, which are evolved in the aqua-regia method, that they started working it for the daily determination of silicon in their pigs. At the present time they use the sulphuric acid method exclusively, and are by practice enabled to work it quite as rapidly as the aqua-regia method. Besides being a neater method, the sulphuric acid process effects no inconsiderable economy in daily practice, where otherwise large quantities of aqua-regia must be consumed.

When using acid water, as in the sulphuric acid method, or strong hydrochloric acid, as in the aqua-regia method, for washing the silica, I make no deduction for filter-ash. The best Swedish filter-paper does not leave any practically estimable quantity of ash when treated in that way. Otherwise, a deduction is made according to Eggertz's formula:

$$\text{Ash, grams} = 0{\cdot}0001\ D^2,$$

D being the diameter of the filter.

Manganese Determination.—For determining manganese in rail steel I use the acetate of ammonia and bromine process, with *final additions of ammonia,* as usual in English and Welsh steel laboratories. In Germany, the method with acetate of soda and bromine or chlorine, and *no final addition of a strong base,* is used, and in Sweden the acetate of soda and bromine method, in accordance with Prof. Eggertz's directions. In describing my mode of operating, I will also try to point out the great differences between the method with *ammoniacal salts and bromine and ammonia combined,* and the methods with *fixed alkaline salts;* and to show what are the precautions to be taken in each case to attain accuracy.

Bromine and Ammonia Process.—3 grammes of steel are dissolved in a flask of 1 litre capacity by aid of aqua-regia; the solution is boiled down, and finally dried. The mass is then dissolved in hydrochloric acid by boiling; water is added to about 750 cc. volume, and the solu-

tion neutralized with ammonia or carbonate of ammonia. If too much ammonia is added, care must be taken immediately to add some hydrochloric acid and to boil for a short time, so as to prevent manganese being precipitated. When neutralizing is completed add 20 to 30 cc. of strongly concentrated, thick acetate of ammonia, and boil until you see the precipitate settle clear after lifting the flask off from the lamp. If the supernatant liquid will not become clear, add cautiously a few drops of strong ammonia (0·88), shake the flask and boil for a moment again. In this way you are certain to obtain a clear supernatant liquid; but you must be very careful not to add too much ammonia, as the manganese may then be partly precipitated as hydrated oxide. After settling, the clear liquid is passed through a filter of 10 inches diameter into a large flask, and finally the precipitate of basic acetate of oxide of iron is poured on to the filter and the remainder of the fluid allowed to filter well off. When no more drops seem to come from the funnel the basic acetate is washed down into the first flask by means of boiling water, and hydrochloric acid is added. The flask is well shaken and heated to boiling, in order to insure the remainder of the manganese being present only as manganous oxide. Neutralizing and precipitation is then repeated as before, and the filtrate added to the first one obtained. For rail steel I find two precipitations like these quite sufficient, the manganese in such steels rarely exceeding 1 per cent. But for spiegeleisen, ferro-manganese, etc., it is certainly desirable to redissolve twice, as the more manganese there is in the substance the more of it will be retained in the iron precipitate. Anyhow, it should be borne in mind that a good boiling is necessary after every re-solution, in order to convert the manganese to *manganous* oxide.

The collected filtrates contained in the large flask are then allowed to cool (this takes only a short time, the first filtrate cooling the second, and so on), about 4 cc. of bromine are added and the flask well shaken, so that the fluid may be well saturated with bromine. It is the safest always to add so much bromine as to have quite a reddish color in the solution. Ammonia (0·88) is then added in excess, and the flask well shaken. At first the solution generally becomes quite colorless, but after continued shaking the brown color begins to be more and more evident, and soon the oxide of manganese separates in lumps. It is then boiled for a few minutes, the precipitate allowed to settle and then filtered off, washed with hot water, dried, ignited and weighed.

It is necessary to have the solution quite cold and a large excess of bromine present when precipitating the manganese in this way. If the solution be hot a violent evolution of nitrogen gas will take place, and the manganese remains in the solution. Insufficient bromine also causes only a partial precipitation. One may write the reactions in this process as follows:

I. *Insufficient bromine:*

$$2[MnO_2(C_2H_3O)_2]+5Br+8NH_3+(x+2)H_2O=(MnO_2+xH_2O)+MnO_2(C_2H_3O)_2+5NH_4Br+2[NH_4O(C_2H_3O)].$$

II. *Sufficient bromine:*

$$MnO_2(C_2H_3O)_2+5Br+8NH_3+(x+2)H_2O=(MnO_2+xH_2O)+5NH_4Br+2[NH_4O(C_2H_3O)]+N.$$

(To be continued.)

DISCUSSION ON STEEL RAILS.

Before the American Institute of Mining Engineers, at the Virginia Meeting, May, 1881.

C. P. SANDBERG, London, Eng.:* I think we should all be grateful to the Pennsylvania Railroad Company, and to their chemist, Dr. Dudley, for spending so much time and money in order to solve an important question; nor are we less indebted to the public spirit which leads them to impart the experience thus gained. In Europe no company or private individual has hitherto done anything similar. In England the great railway companies employ their engineers in other ways, and do not keep a chemist or specialist to study rails; but even if they did they would probably not publish the results. The chief professional engineers are so occupied with their private practice of railroad construction generally, that they cannot be expected to devote their time and energies in so special a question. The German Railway Union has, in my opinion, missed the real object of their elaborate researches by falling into the error of specifying costly impracticable tests for rails—tests at the same time which do not infallibly expose the impurities which may be in the rails under examination. There has, therefore, been nothing done on this side of the Atlantic in the way of exhaustive study to determine the best composition for steel rails. America should consequently have full credit for the enlightened example which she has set.

* Sent to the Secretary in manuscript.

I wish, however, to make a few remarks on Dr. Dudley's second paper—a paper which follows very much upon the same lines of argument as the first, read three years ago, upon which I have previously touched in my paper on the same subject read at the meeting in August last. If Dr. Dudley had discovered a new metal he could hardly, I think, have taken more pains to prove that he was correct in his first formula for the best chemical composition of steel rails. But I have seen so much done in the way of proof by experiment that I am inclined to think that almost anything can be proved by experiments and by samples. If Dr. Dudley would only give up his formula, which would not suit any country—not even America—to work by the ordinary Bessemer process, and would satisfy himself by applying such chemical tests in comparison with mechanical or physical tests, which are practically workable, with a view to arrive at something better in rails generally, he would, I think, have done his company and society at large better service than even he has so far rendered. As it is, he seems to have the American makers arrayed against him, and I fear that the European makers will not sympathize with him very much either. No one can, however, deny that Dr. Dudley has carefully worked out a series of experiments, sparing no pains to substantiate his views. Still it by no means follows that his conclusions are correct, and, as has often happened before, doctors may honestly disagree as to the best means of arriving at the truth.

As to the discussion at the Philadelphia meeting, it is to be regretted that it chiefly represents one side, viz., the makers; for the engineers do not seem to have been well represented at the discussion. If they had been better represented the general conclusion would no doubt have been different. Some of them would probably have agreed with Dr. Dudley in many respects where the makers were against him; for instance, they would have held that both copper and sulphur are of more importance to the producer, and, therefore, the consumer can safely leave them out of the question without detriment to himself.

But for blooms it will be necessary, at least, occasionally to determine the sulphur so as to detect red-shortness which might otherwise cause cracks and wasters in rolling out the English blooms in America. This would be particularly the case if the mills used for that purpose had low speed, so as to prevent the steel being worked out at

a good heat. As for copper, an excess is more rarely the case; moreover, a larger amount may be tolerated than in the case of sulphur, so that this determination is not of such importance as that of the sulphur. In the basic process the sulphur is of very great importance, as the ordinary white pig iron generally contains a greater amount of this impurity than does the Bessemer pig iron. It is, however, to be hoped that by increased experience makers will succeed in the elimination of sulphur as well as they have done with the phosphorus. Anyhow, sulphur will always be a source of trouble to the maker rather than to the consumer of rails.

The carbon test by coloration according to Eggertz's method is quite reliable within any limits that occur in rail-steel. It is true that it does not show the graphite, but the amount of this is so small that it can be left out without any detriment in practice. Besides, this method is very generally adopted, and where the make is large and every charge has to be tested, the number of tests may amount to about one hundred a day. How could this be done if combustion tests were used? Moreover, I should mention that Professor Eggertz has lately introduced considerable improvements, by which greater accuracy can be obtained even when the carbon is very low.

No one has a right to doubt the correctness of analytical results made by Dr. Dudley's assistant as long as he certifies to them. But the makers have, no doubt, some right to complain of the small amount of silicon allowed in Dr. Dudley's formula, viz., 0·4 per cent. What would Dr. Dudley say if, on analyzing some steel rails from his own road, which had given very good results, he had found ten times as much silicon as his formula required—a result which I think quite possible, as I have sometimes found this amount of silicon in steel of excellent physical character. There is no impurity in steel rails which may vary so much as the silicon, not only in different districts and countries, but even in the same works. It often varies four or five times as much at one time as at another, the variation depending chiefly upon the heat of the blow (the pig iron used being the same). Silicon passes away simultaneously with the carbon in the cold blow, but keeps in almost undiminished quantity in the hot blow; and the makers have as yet no means of determining when the silicon is removed, as they have, for instance, in the case of carbon. The use of the spectroscope has been of but little value in practice on a large

scale. It would therefore be very hard—in fact, quite unnecessarily severe—to limit this impurity to such a degree as to be scarcely attainable in practice.

I fear also that Dr. Dudley's physical tests are quite as impracticable as his chemical ones. To apply a bending or a shearing or torsion test to an article which has in daily use to sustain concussion and abrasion seems a curious course of procedure. Besides, these tests are too slow and costly to be of much service in actual practice, just as is the case with the tests of tensile strength and contraction of area stipulated by the German Railway Union.

I have had ample experience of late to prove the comparative worthlessness of such tests in Germany, and I should regret to see America or any other country adopt them. For years these tests have been insisted on by the German Railway Union for all their rails; the drop test has been so reduced as to have little or no effect, and even in some cases it has been abolished altogether. Since I commenced inspection at German works, not only have I found some makers objecting to my heavy drop test, but on analyzing the German rail steel I have found that it contains twice as much phosphorus and silicon as the English rail steel, where the drop test has principally been used for many years. In fact, the scientific German tests, with all their disadvantage of slowness and expense, have proved altogether less efficacious in keeping out these impurities than the simple drop test has been, although the latter is designated as "crude." Still Dr. Dudley recommends the former when he cannot put in execution the still-born idea of a registering manometer on the punching machine for measuring the hardness of the metal, as suggested at Barrow many years ago, but never actually put in practice. Why should we give up a test that has for years done good service for millions of tons, simply because it is "crude," and adopt a slow, costly and impracticable one (which has not done good service), simply because it is "scientific"?

As to the theory of the softest rail being best for wear, I should require further proof on thousands of tons before positively accepting it. If the Pennsylvania Railroad Company and Dr. Dudley will, as we hope, continue their researches on steel rails, not only for their own benefit, but also for the good of mankind, I would venture to suggest that, instead of repeating for the third time these costly and elaborate, not to say tedious experiments, they should lay a thousand

tons of soft rails, made to Dr. Dudley's formula and proposed physical test, and also, on the same line of road opposite, another thousand tons of hard rails, with the ordinary chemical composition, inspected as I have described, so as to compare the results of wear, with the view of proving which is the best. This would cost little or nothing, and after all be more convincing than any experiments, however carefully made, on single bars, such as those now executed with so much care. The only drawback to my suggestion is the time it would require for the comparison, this time depending upon the amount of traffic on the line where they are put down. Meanwhile some approximate results from single rails could be obtained, as to the wearing resistance of soft and hard rails, and the hardness derived either from carbon, phosphorus or silicon, if a locomotive engine were placed on such experimental rails, one being soft and one hard; it should have its driving-wheels sliding on the same spot, water and sand being applied, and the cutting or wear in the two rail-heads could be measured, or the reduction of weight ascertained. This mode would have the advantage of giving results in a day or two, but I admit that it would be but a crude test of single bars.

Ten years ago a paper was read at the Institution of Civil Engineers by a Mr. Price, of Dublin, on a rail-testing machine very much like a turn-table, which was turned round on experimental rails with high velocity, in order to ascertain the resistance against wear for rolling weight; but this machine never came to work, at least not in London, where it was meant to establish it as a public testing-machine for rails. The best testing-machine is now the underground railway, where steel rails of a very heavy bull-headed section last but a few years; but it is doubtful whether the engineers in charge would allow it to be used for experiments, at least they have not done so yet. The best authority on this subject is Mr. R. Price Williams, M.I.C.E., who has read several papers, and collected data and statistics on the wearing resistance of the rails on English railways.

In the address to the meeting of the Iron and Steel Institute, which has been just held in London, the President, Mr. J. T. Smith, of Barrow, quotes Mr. Price Williams' experience of steel rails lasting nine times as long as iron rails, but adds that this is probably more often the exception than the rule; and the past President, Mr. Menelaus, thinks the endurance to be only three times that of iron

rails. I have of late years adopted six times, but since the price of steel rails became reduced to that of iron no calculations are needed to show their preference. It is, however, certain that those steel rails from which such excellent results were obtained at first, say ten years ago, were made harder than they are now, and this seems rather to oppose Dr. Dudley's new theory that the softest rail lasts the longest.

An alteration of importance in the second paper is that the Doctor in his third test has adopted the use of crop ends, although he still adheres to bending tests instead of drop tests for the sake of securing a soft material. But the drop test would also secure a soft material if prescribed to a minimum deflection for a certain blow given; and at the same time it is a criterion of safety. I have stated that as an effect of my standard drop tests the deflection should amount to 3″ or 4″ according to the hardness of the steel. I admit that the difference in the foundation, as to solidity, might be misleading, but, at any rate it is good enough for comparative tests made in the same place and for the same section; besides it has the advantage over the proposed mode of cutting out the test-piece from the rail and bending in the machine, a test which is too slow and costly to be used daily in practice.

With a view of securing soft material for rails the basic process comes in most beautifully; by this process they can be made as soft as lead and with only a trace of silicon, and still the ingots are made solid, no doubt for the reason that the basic steel is cast so much hotter than the Bessemer.

In fact, the difficulty by this process was at first to make steel of ordinary hardness; but now, since hematite pig is added instead of spiegel, steel of any degree of hardness can be produced. Indeed I am just now inspecting blooms made by this process, and judging from several analyses made, I have found the average contents as follows: silicon only a trace, carbon, average, 0·33, manganese 0·35, and phosphorus 0·08. Such rails, tested to my standard drop test, show a deflection of about 5 inches, which indicates more softness than ordinary rail steel made by the Bessemer process. The safety from breakage secured by the soft metal is decidedly an advantage to the engineer, but not exactly so to the railmaker, since the softer metal necessarily gives more cracks and wasters than steel of medium hardness.

Having been the inspector to the Swedish government railways for the last twenty years, I have been naturally anxious to secure to them a rail which should be safe against their rigorous climate, and fearing that the so-called phosphor-steel would break, I made, at the Panteg Works, in South Wales, six years ago, some special phosphor-steel rails (about one hundred rails, with the amount of 0·25 per cent. phosphorus, but low in carbon and silicon), and tested them to my standard drop test.

These rails have been down at Stockholm on a siding ever since; none of them have broken, although the cold sometimes reaches minus 30°F. I can also give the experience from the Swedish state railways of other rails, with a composition of carbon, average, 0·20 to 0·30 per cent., phosphorus 0·06 to 0·12 and silicon 0·10 to 0·30 per cent. Out of 35,000 only four rails broke during the winter of 1880, and these breakages might be equally well caused by mechanical force as by the chemical composition. However, it is of course safer for cold climates to have as little phosphorus as possible in the rails, and to secure hardness by the presence of carbon.

The results just given show how wide a variation in the composition can be tolerated, even for rails exposed to a most rigorous climate. I think we ought to be indebted to Dr. Dudley for having raised the question of chemical composition, for the more we go into the matter the more our views will be widened, and we shall be disposed to grant more liberal variations in the impurities than we should have done if we had not analyzed the metal. Before chemistry has enlightened our views, there will naturally be errors committed from "*trop de zèle*" on the part of the engineers. For instance, an eminent engineer, not long ago, rejected some steel rails because they contained 0·15 per cent. of silicon, although they were faultless in all respects.

Another question arises as to the wearing resistance and hardness derived from carbon alone, compared with that from phosphorus, silicon or manganese, carbon being, in these cases, at a minimum. Whether the wearing resistance is the same or not, we know that safety is best secured by the carbon hardness, and by a minimum amount of the other impurities, particularly in countries with cold climates. However, in other countries with mild climates, and where the ores are impure with phosphorus, millions of tons of rails have been made, and laid down on the track, which seem to answer well,

both as to safety and wear, although the carbon must be low, with a rather high proportion of phosphorus. Since, by the discovery of the basic process, the phosphorus can be eliminated and the carbon added by using pure hematite pig instead of spiegel after the blow, there is an opening even for the countries with hard climate, and with impure ore, to produce good rail steel with a minimum of phosphorus, and with hardness resulting from carbon, just as if the steel had been made originally from pure ores. I should also remark that the amount of manganese—0·35—given according to Dr. Dudley, might well be doubled as a means of lessening the other "deadly sins of impurities" —to use Dr. Siemens' expression—and also in order to facilitate rolling into clean rails.

I would sum up my remarks on Dr. Dudley's paper by saying that, while I disagree with his tests, both chemical and physical, I nevertheless agree with him in the application of chemistry to steel inspection; but it must be used only in a practical way; and if I have, in my paper read before this Institute, shown any decided preference for mechanical tests, it is simply because they have, under ordinary circumstances, done good service, and proved quite sufficient in practice. I have also said, "Let the chemist help us, but not be our dictator." For my own part, I apply chemical combined with mechanical tests, and I have in that way even carried out inspection under Dr. Dudley's specification where railway companies insisted upon adopting it. Mechanical tests come first, and the drop test to my standard is insisted upon for both rails and blooms (sample blooms are rolled into rails for the purpose of being tested); borings are then taken from the steel actually tested mechanically, and sent to my laboratory, where the carbon, phosphorus, silicon and manganese are determined.

However, I do not think it advisable to publish any particular formula, or maximum and minimum of composition, because we are really not yet in a position sufficiently assured to do so. I think the first step in introducing chemistry into rail and bloom inspection is to institute a combination and comparison between chemical and mechanical tests. After sufficient experience and reliable data have been obtained the time will have arrived when exact formulas may be established.

In the inspection of rails I only analyze when the mechanical test gives any extraordinary results. If, for instance, a rail has

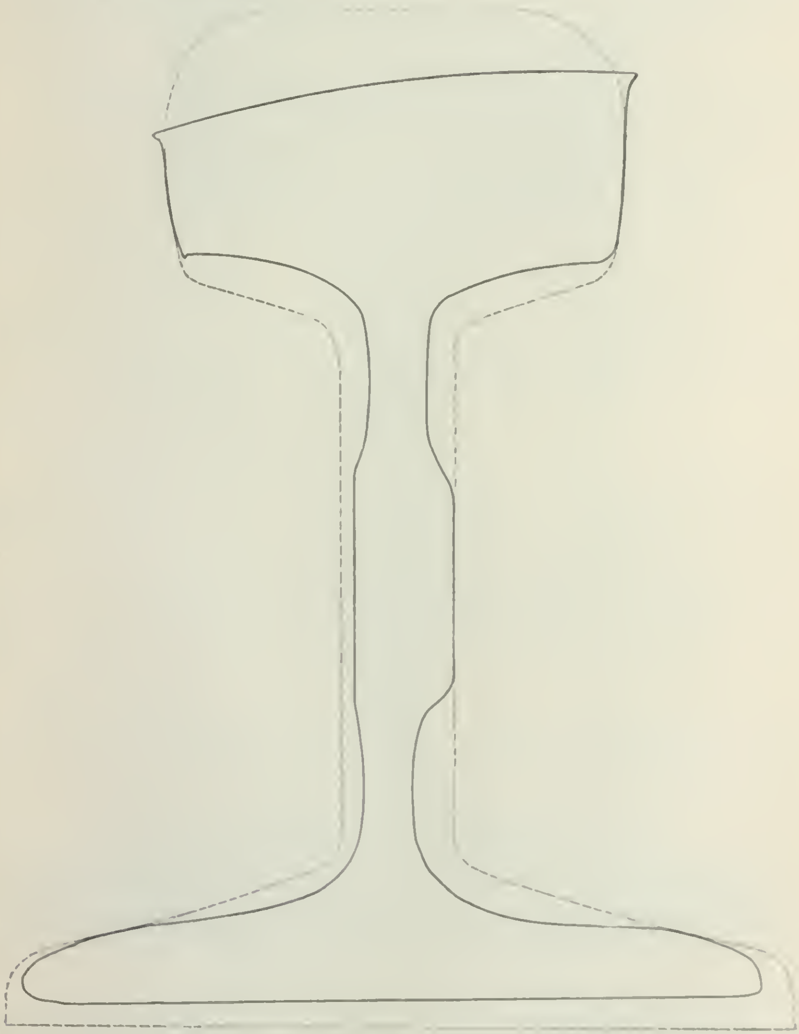
broken under the falling test, if it has broken on the road under service, if it bends differently, showing different degrees of hardness, then the chemical analyses are of interest, and may be of service. After all, however, it is only a large number of trials that should count, and no decided judgment should be formed from single observations. Chemistry has been very useful to the maker, and may be equally so to the engineer when properly applied; but no positive deductions should be made except after large and varied experience and experiments.

I think I have shown that, as a professional inspector, I have, at any rate, done my part towards introducing chemistry into my practice wherever railway companies and my clients specify such tests. For their own sakes, as well as for the solution of many questions now under discussion, it is to be hoped that purchasers, without specifying too much, will rather proceed by degrees to adopt chemical inspection of their rails and blooms, in order to avoid too great conflict. The engineers should never lose sight of the fact that the solution of such a question as this would be arrived at much more safely, and much better, with the assistance of the makers, and therefore it is not expedient to vexatiously oppose them. On the other hand, the makers should bear in mind that they cannot have it all their own way, and I may safely say that they have *not* got it in Europe.

I agree, without reservation, with the views which my friend, Mr. A. L. Holley has expressed in his paper on "Rail Patterns"; and I can prove, by the contents of my circulars, and by the correspondence in technical papers, which has from time to time taken place, that the opinions I held when I took up the question of standard or normal patterns have remained unchanged, and are identical with those referred to above. It is, however, evident that occasional remarks on the subject, published here and there in various ways, are not so valuable and serviceable as when collected and issued in a complete and concise form, and it is such a work of reference on the subject of rail patterns that is now available in Mr. Holley's paper.

Rail making is a comparatively new branch of the iron and steel industry, yet it is of great importance, and railmakers have every inducement to improve their machinery, so that in course of time more difficult sections will be rolled with ease, until ultimately, perhaps, a thinness will be obtained comparable to a spider's web.

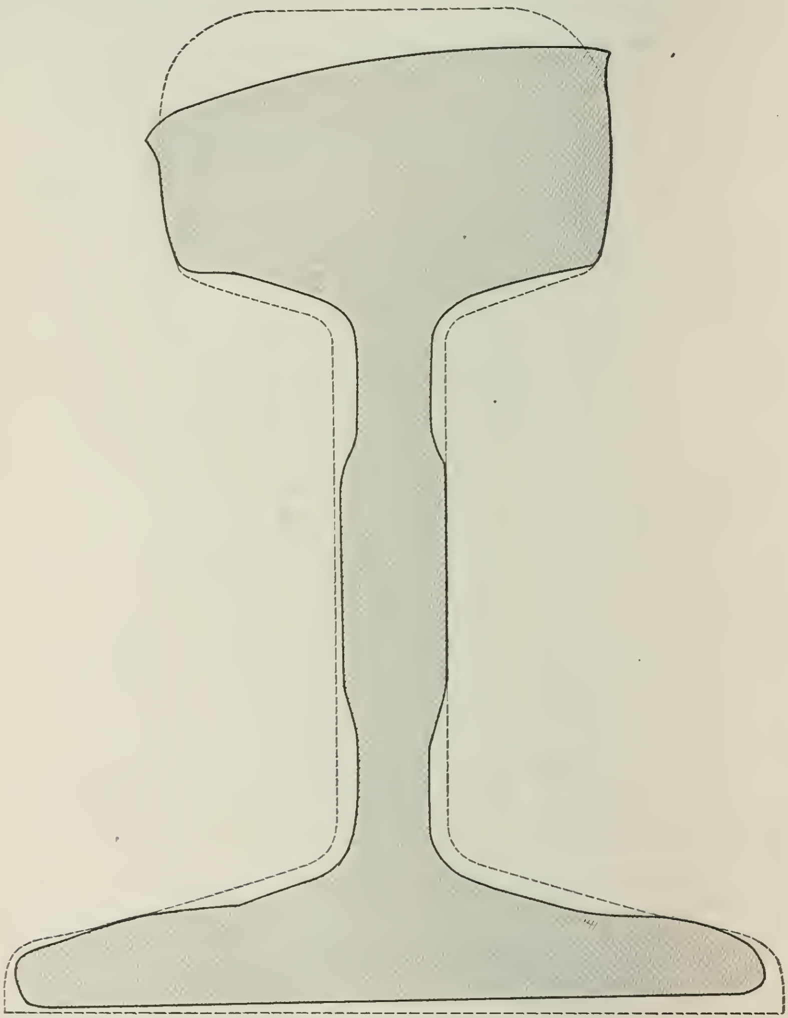
On the other hand, just to show that there is some "body" required even for the flange and web of the rail to resist the wear and tear at the joint, particularly if it is loose, I inclose a lithograph drawing of a steel rail after eight years' service on one of the best maintained railways in Germany. (See accompanying cuts.)



End View.

But it necessarily follows that, with this progression, normal forms and patterns will only be retained as standards for periods of greater

or less duration; and it will become a question for the consideration of the railway engineer whether it would be more advisable to retain and alter what he has, or to put up with the inconvenience



Section beyond the second bolt-hole.

of keeping several patterns and having new fish-plates. In the construction of new lines one can choose the most modern and economical section, only limited by the price at which it can be obtained. Under these circumstances, it is evident that no patterns will be uni-

versally adopted forever, notwithstanding the vast amount which would be saved if they could possibly be definitely fixed. It is curious to remark that the very man who now proposes to finally settle upon a standard is, probably, the one who by his genius, aided by the immense resources of the United States, will produce more desirable patterns and sections than those which he would now like to make permanent, and thus bring about the changes which he now deprecates.

I willingly admit that, considering the present stage of excellence to which rail making has attained, my pattern appears "clumsy" alongside of the more difficult one of Mr. Holley's. In 1870 I first designed my pattern in accordance with what English railmakers could then produce, and with the end in view that no extra cost was to be incurred in manufacture. In 1878 I found that the mills were so far advanced that there was a decided call for a second series of patterns, and it was not to be expected that either of these series should be equal to the Holley pattern, recently designed, with respect to the flanges and web. The difference is merely an illustration of the progress which has meanwhile been made.

Mr. Holley's new patterns are produced after the vast experience gained by him in his visits to railmakers in Europe for newly designed and constructed works in America. It may be remarked here that the European mills have much improved of late. New mills have been laid out in England, partly on the reversing system, and on the Continent the improvement is marked by the use of three high rolls.

With reference to Mr. A. Welch's remark and also Mr. Hart's letter, about the first original section for steel rails designed as early as 1866, I wish to assure both these gentlemen that I had not seen the patterns when I designed mine, and even if I had, I should not have then dared to put it forth as a standard for English railmakers; and the soundness of my opinion has a simple but conclusive proof in the statement made concerning the extra price which was at that time required for their production. I need not say that I highly approve of the original Welch section, for it is in principle similar to the design which I made fifteen years subsequently; and from a professional point of view I would fully concede to Mr. Welch the credit of first designing the modern steel rail section. From a commercial, matter of fact and practical point of view, he was, however,

before his time by so many years; or, to put it in another way, rail making was ten years behind its time when the new metal steel was first introduced for rails.

If his design has been incorrectly identified with my name (as the sections are similar in principle, though they differ in thickness of flange and web), I beg to say (1st) that I much regret the misunderstanding which has so arisen through no fault of mine, and (2d) that no confusion has been intentionally created by me.

It is a pity that the section designed so long ago has not been before made public, as it would make a really good standard pattern, and even now quite equal to present requirements.

In conclusion, I wish to say that I have never desired to claim any invention or novelty by the publication of my sections; they were put forth as being such as English railmakers could and would produce at the ordinary market price, and as being the best and most suitable to be so obtained. Had Mr. Welch or Mr. Holley put forward theirs ten or fifteen years ago, I would have been the first to recommend them. In fact, I should have been glad to see their names associated with rail patterns in their own country, in the same way as mine has been.

PROF. RICH. AKERMAN, Stockholm, Sweden:* I agree with your (Mr. Sandberg's) views, that it is too early yet to stipulate only one certain chemical composition in the rails. Such a stipulation, I think, might be justified, if it was not for the manganese; but this metal changes the properties of the iron and steel in the same direction as carbon, silicon and phosphorus, by increasing its tensile strength, stiffness, hardness and brittleness. If the iron and steel does not only contain carbon and manganese, but also phosphorus, silicon and sulphur, I think everybody will agree that the manganese, to a great extent, can neutralize not only the effect to red-shortness, caused by the sulphur, but also the tendency of the iron or steel to become burnt or brittle, caused by the phosphorus and silicon. Thus brittleness can be as well increased as decreased by the presence of manganese, and I, therefore, consider that, however necessary analyses now and then may be, in order to enlighten us and to explain exceptional cases, yet they are not convenient as practical tests upon rails, especially as the properties of the rails not only depend on the chemical composition of

* From a letter to Mr. Sandberg.

the iron or steel, but to a great extent also upon the soundness of the ingots and the mode of manufacture.

Still less necessary than analyses I regard the German system of controlling the rails by researches on contraction of area. I do not consider either the elongation or the contraction of area, in fracturing samples, to test tensile strength to give a correct idea of the toughness of iron or steel. On the contrary, I regard the falling test, properly carried out, to be much better in this respect and to give a greater safety against breakage than the common tests on tensile strength.

Dr. R. W. Raymond, New York: It occurred to me after the presentation of Dr. Dudley's interesting paper on this subject, and after the discussion to which it gave rise, that the important data he had collected might be made, by suitable mathematical treatment, to yield more significant indications than had yet been obtained from them. It is true that Mr. Cloud has discussed by the method of least squares, the results set forth in Dr. Dudley's former paper, on the breakage of rails. But the breakage of a rail is, I think, far more likely to be due to mechanical or accidental (not chemical) conditions than the loss of metal by wear. For this reason I should attach less importance to the data, and place less reliance in their discussion. Moreover, the number of observations in the paper referred to was relatively small—too small to permit the method of least squares to give trustworthy results. Finally Mr. Cloud correlated the chemical constitution with certain physical tests, not directly expressive of the resistance of the rail to breakage. In the case before us we have sixty-four observations, and they directly connect certain chemical proportions with a definite physical result of experience, namely, the loss of metal by wear. I think, therefore, that this case presents a better opportunity for such a mathematical discussion as will test, to some extent, the assumptions underlying Dr. Dudley's conclusions.

These assumptions are: that the loss of metal per million tons of traffic depends, first upon the circumstance under which the rail is worn; secondly, upon the proportions of its various constituents, and that the amount of loss is affected by each constituent in a certain ratio, so that if the circumstances were the same for all the rails, the wear per million tons might be approximately expressed by an equa-

tion of the form $I + Cc + Pp + Ss + Mm = R$, in which I is an unkown value, depending on the quantity of iron (and other ingredients not determined) in the rail, and assumed to be constant for all the rails; c, p, s and m are the experimentally determined quantities of carbon, phosphorus, sulphur and manganese, as tabulated in Plates VI and VII of Dr. Dudley's paper; and R is the experimentally determined wear per million tons; while C, P, S and M are unknown coefficients. In other words, it is assumed that the difference in wear is proportional to a difference in c, p, s or m, and that the effects of a change in the amount of either are independent of the amount of the rest—within the experimental limits of the problem.

It is also assumed that sulphur, copper and other ingredients, not determined, are equally present in all the rails, or at least do not affect the wear, since I is taken as constant.

Dr. Dudley tacitly assumes the coefficients C, P, S and M to bo positive; and assigns certain values to them, by means of which he reduces the equation to the form $I + Px = R$, x being equal to $\frac{Cc + Ss + Mm + Pp}{P}$, or the number of so-called phosphorus units in the rail.

But we do not have R directly given under constant circumstances. The varying weights (per engine, car or train) and the varying speeds we must neglect for lack of data, and assume that in these respects the rails examined have been treated substantially alike. But there remain the varying conditions as to track, which must be eliminated. Dr. Dudley gives six classes, four of which contain 8 rails each, and two 16 rails each. Beginning with the first (tangent grade), we take the mean of the figures expressing wear for that class; and continuing this process through the other five, we obtain six expressions in quantity of loss by wear, which we assume to represent, in their differences, the varying conditions of the track, with respect to grade and direction. We now take the mean of these means (giving double weight to the groups containing 16 observations). This general mean is the loss of metal per million tons of traffic for a rail of average constitution under mean circumstances. The difference between it and the mean for any one group is a constant correction to be applied to the tabular wear of each rail in that group, to obtain what may be called the reduced loss for that rail; that is to say, an expression for the loss per million tons of traffic which that rail

would have shown, had it been worn under average circumstances. For example (the tabular numbers being multiplied by 1000, to get rid of long decimals), the general mean of loss is 76·7; the mean for the tangent-grade group is 70·1; the correction is + 6·6; the experimentally determined loss of rail 887 (of that group) is 38·6; and the reduced loss, 45·2.

I had at first intended to do with the units of wear what Dr. Dudley did with his chemical units—reduce them all to one actual group. As he obtained phosphorus units, so we might obtain tangent-grade, or grade-curve or level-curve units. But the reduction to an ideal average unit of wear is preferable, as giving the smallest possible average corrections, and distributing the errors involved in the corrections uniformly over all the observed cases, instead of concentrating them on a part. I am indebted for this suggestion, and for the larger part of the voluminous calculations required in the details of the work, to a mathematical friend, without whose aid I should scarcely have been able, in the scanty intervals of other absorbing occupations, to solve the problem, even after stating it.

It is, however, simple enough, though very tedious. From the 64 given cases, we form 64 equations of condition, of the form $I + Cc + Pp + Ss + Mm = R$, in which the different symbols have the same meaning as before, except that R is the reduced loss by wear, as already explained.

These 64 additional equations are now, by the method of least squares, reduced to 5 normal equations, containing the five unknown quantities, I, C, P, S and M. The solution of these equations will give us the most probable values for I, C, P, S and M which the equations of condition can yield.

I do not intend at this time to publish the details of the calculation. If time permits, there are other assumptions which I should like to introduce into the discussion, to test the results already obtained. It would be interesting, for instance, to repeat the whole process with equations of the form $I \times Cc \times Pp \times Ss \times Mm = R$.

At all events I am not now prepared to present the figures of the calculation, in which, though the equations have been solved, the probable errors of the solutions have not yet all been determined. But I am able to say that Dr. Dudley's own data and assumptions, thus treated, show the coefficient of silicon to be negative, and that of manganese to be practically zero. In other words, the silicon,

though a "hardener," does not, like carbon and phosphorus, increase, within the limits of these tests, the loss of metal by wear; on the contrary, it increases the wearing capacity. And manganese seems to be neutral in that respect, behaving like so much iron. These two metals play, then, a very different part from that assigned to them in Dr. Dudley's formula, which holds true (and that with modified coefficients) for the non-metals, carbon and phosphorus only.

I will not here enlarge upon the imperfections inherent in the application of the method of least squares, and in the necessary employment of so many assumptions. In spite of them all, I believe that the indications here afforded are significant. Certainly the "phosphorus-unit" system must be abandoned, so far as silicon and manganese are concerned; but are there not hints of practice pointing in the same direction, as the results of this mathematical inquiry, as to those two elements? Have we not heard repeatedly of high-silicon rails showing extraordinary wear? And is it not "important, if true," that manganese may be increased beyond Dr. Dudley's limit, if it be otherwise useful or convenient, without injury to the wearing capacity?

Dr. C. B. Dudley said that he had listened with much interest to the present discussion, but he was not prepared to reply at the moment to all that had been said in criticism of his paper read at the Philadelphia meeting. At a subsequent meeting, after he had had opportunity to carefully examine and study the remarks of the different contributors to the discussion, he would briefly sum up the case from his standpoint.

New Model for Vessels.—Prof. Pictet is experimenting with a new model with the view of obtaining a great increase of speed at sea. The keel is so shaped as to reduce the resistance of the water, and the vessels, instead of sinking their prows when the velocity increases, raise them so as to diminish the friction and glide over the water. He expects to attain a speed with steamboats of more than 30 miles an hour. A boat is now in process of construction at Geneva, and experiments will be made with it upon the lake.—*Les Mondes.* C.

NECESSITY OF CLEAR MECHANICAL CONCEPTIONS.

By William D. Marks,

Whitney Professor of Dynamical Engineering, University of Pennsylvania.

The nomenclature of mechanical conceptions is a matter of common consent, and could soon be reduced to approximate uniformity were the majority of writers upon mechanics sufficiently painstaking and clear in their use of terms, and would they bear in mind that a clear physical conception of the meaning of terms used is of vastly greater importance than any subsequent display of skill and ingenuity in the mathematical manipulation of the symbolical expressions for them.

Every term used in mechanics should convey to the mind a distinct physical conception, capable of being expressed in intelligible language without recourse to symbolical notation; and until this fact is recognized and acted upon, mechanics will ever be a dreaded study to those who are forced to take it up, saving that small proportion of students thoughtful and patient enough to elaborate their own conceptions by careful decomposition and isolation of the elements of the symbolical expressions which are taken for the foundation stones of an elaborate mathematical structure.

None who are engaged in teaching can have failed to perceive the stupefying effects of a course of symbolical reasoning unaccompanied by any attempt to materialize the meaning of the expressions deduced, or have not noted the injury of a naturally clear intellect in the attempt to memorize a mass of partially apprehended formulæ.

Great mathematical acquirements do not seem to be an absolute necessity, since discoveries in natural philosophy seem to point out new and appropriate methods of quantitative treatment rather than to render available the labors of the pure mathematicians.

Our knowledge of mathematics does but enable us to weigh and measure our results. Every new mechanical problem should first be analyzed by means of a course of abstract reasoning before being quantitatively analyzed with the aid of mathematics, just as a cautious chemist precedes a quantitative analysis by qualitatively determining the nature and ingredients of the substance under consideration.

Clear ideas of the meanings of mechanical terms are imperatively

required as a first condition of success in a preliminary analysis of any problem, and any attempt at quantitative analysis will more probably lead to error than truth unless this preliminary analysis be complete.

That the writer may not appear to have "set up a man of straw" for the pleasure of demolishing him, he will instance a few cases occurring in the works of the abler writers upon mechanics, passing over without notice the too apparent evasions and misconceptions of a host of writers of so-called "elementary mechanics."

Prof. Wm. Whewell, who is probably the clearest writer in the English language upon mechanics, constantly uses the terms force and and pressure interchangeably. Pressure refers rather to force distributed over a considerable surface, as in the case of water, the atmosphere, etc.

Prof. Rankine calls the moment of inertia of a revolving body the weight multiplied by the square of the radius of gyration; this expression is not the moment of inertia, but only *the measure of the moment of inertia.*

Prof. Tyndall defines heat as "a mode of motion"; it is really a form of work. Possibly this apparent error is a wilful misstatement, made with a design to convey to his readers an approximate idea of what he did not believe them capable of conceiving fully. It certainly is either an error or a concession to ignorance, which has done much harm.

It is very much easier to criticize defects than to remedy them, and the author, in offering the following verbal definitions, does not feel that he has made himself as clear as he could have wished to be.

He trusts, however, that they will serve the purpose of showing more clearly the meaning of the usual terms of mechanics, and their relations to each other, giving in the present form of successive aphorisms a connected view of the whole field of mechanics.

In order to be perfectly clear, and establish a complete understanding between our readers and ourselves, we will have to repeat the most elementary ideas, because the terms having the more complex meanings will demand the most precise accord as to the meaning of the elementary terms to which they will be reduced. We may, then, be pardoned for the repetition of definitions with which all are assumed to be familiar.

Dynamics may be separated into two studies—kinematics and

statics. When these two are considered in conjunction we have dynamics.

The careful isolation of these two branches of dynamics, and their separate study, will add much to the power of apprehension of the student when he comes to consider them conjointly.

Kinematics evades all questions of force, and in it we confine ourselves entirely to the consideration of the path, velocity and direction of motion.

Motion can best be defined as a change of position, and in many cases the velocity of this change is a matter of indifference, so that the path and direction of motion only receive our consideration. If velocity is taken into consideration we introduce the element of time, since the velocity of a point is the distance which is [or would be if the velocity was constant] passed over in whatever unit of time is used as a standard. One second is the usual standard.

We can then say in uniform motion the space described in any time is equal to the product of the velocity and the time.

When the velocity is not constant it can no longer be measured by the quotient of the space by the time, since these quotients will be different for different periods, and in variable velocities we measure the velocity, at any instant, by the space which would have been passed over in the succeeding second had the velocity been rendered constant at that instant.

Angular velocity, which is used to compare the speeds of rotation of bodies around their axes, can also be constant or variable; it is the velocity, in a circular path, of a point which is at a radial distance equal to unity from the axis of rotation of any rotating body; or, if the axis does not pass through the body, it is the velocity, in a circular path, of a point situated at a distance from the axis equal to unity, and in a an assumed line joining the axis and the body revolving around it. It can always be obtained by dividing the curvilinear velocity of any point in a rotating or revolving body by its radial distance from the axis.

Equipped with these few fundamental conceptions, we have all that are necessary to the study of kinematics, this word being used in its most limited sense.

Statics, on the other hand, evades all questions of motion, and in it we confine ourselves to the study of forces at rest.

What force really is we will probably never know until we learn

the ultimate nature of matter; we do know, however, that whatever tends to produce motion, or actually produces motion in bodies at rest; or brings or tends to bring a moving body to rest, or to change the direction of a moving body, is called force. We measure force by its intensity, in pounds, and limit it by its direction and point of application.

In terrestrial mechanics, to which we limit ourselves in this paper, gravity is the force which attracts all bodies to the surface of the earth in a vertical line; if allowed to act on a free body in a vacuum it will produce a velocity of about 32·2 ft. at the end of one second, during which time the body will have fallen a distance of 16·1 feet.

In order to define the centre of gravity of a body we will have to precede it by the definition of the statical moment of a force which is the intensity of that force multiplied by its perpendicular distance from the point around which it tends to, or actually does produce motion.

We can now define the centre of gravity of a body as that point which, if supported, leaving the body free to rotate in any direction, would balance all the moments of the forces of the molecules of the body due to the force of gravity; the body would not have any tendency to turn about this point, at which the total force of gravity acting upon the body may be assumed to be concentrated.

The well-known theorems of the parallelogram and parallelopipedon of forces form the basis for the statical treatment of forces which has received an enormous development both analytically and graphically.

We come now to dynamics, which is the study of combined force and motion, that is, of work; or if time in which the work is accomplished is included in the consideration, of power.

We can say that work equals force multiplied by the space passed over during the action of the force, and as the unit of space usually assumed is a foot and the unit of force a pound, we measure work in foot-pounds. A foot-pound is the amount of work done in raising a weight of one pound one foot, or in exerting a force of one pound through a distance of one foot in any direction. Work is considered independently of the time in which it is accomplished.

Power is work considered with respect to the time in which it is accomplished; as, for instance, a horse-power, which represents 33,000

foot-pounds of work done in one minute or 550 foot-pounds of work done in one second.

The many terms used to express the idea of force and motion combined can all be seen, by a little thought, to be synonymous with work. Power is often used incorrectly for force when the lever and screw are being discussed.

The weight of a body is the measure of the intensity of the force of gravity acting upon it. In treatises on mechanics it is made equal to the product of its mass by its velocity at the end of one second (32·2 feet) under the action of gravity.

In order to clearly grasp the meaning of this last sentence we must know what mass is. The mass of a body is usually stated to be the quantity of matter in it, and it will at once be perceived that the hypothesis is placed that differences in quantity of matter make proportional differences in the weight, which may or may not be true. We have no means of proving that a volume of iron which weighs 7·2 times as much as the same volume of water contains 7·2 times as much matter.

The fact is that mass means the intensity of the force of gravity divided by the velocity due to the force of gravity at the end of one second, and is a constant ratio at all points on the surface of the earth. The great convenience of this ratio for the purposes of the mechanic will be seen when we recollect that in dynamics the intensity of a force is measured by the velocity which it will produce in one second, and if we multiply this ratio (which is the mass) by the velocity which is observed, we have the intensity of the acting force in pounds. This leads us at once to the momentum of a body, which is the intensity of a constant force which has been (or should have been to produce the same velocity) acting upon it for one second, it is equal to its mass multiplied by its velocity, in feet per second.

If a moving body be brought to rest in one second, the momentum is the intensity of the constant force which must be exerted through a space equal to one-half the velocity of the moving body.

In speaking of momentum, unity of time is always assumed as one of the conditions; thus the weight of a body equals its momentum when gravity is the force acting upon it.

The distinction between momentum and the really acting force in many cases which occur must be sharply drawn. If the acting force be constant, it will add equal increments of velocity in each unit of

time, and the intensity of the acting force can be at once deduced by dividing the momentum by the time of its action. Thus a body let fall and acted upon by the constant force of gravity has at the end of one second a momentum Mg, and at the end of two seconds a momentum of $2Mg$, but the acting constant force is still Mg or $2Mg$, divided by the time two seconds.

Thus we have the distinction, the momentum (Mv) is the intensity of a constant force which acting contrariwise upon any mass in motion, with the velocity v for one second, would bring it to rest; or, if the mass (M) be at rest, will in one second impart to it the velocity v.

On the other hand, the acting constant force $F = \frac{Mv}{t}$ may act for any length of time, t, and therefore may be of any intensity.

Work has already been defined as force (F) multiplied by space (s). If now for F we substitute $\frac{Mv}{t}$ and recollect that for any free body put in motion by the action of a constant force, the space passed over is equal to one-half the final velocity multiplied by the time, we have:

$$\text{Work} = Fs = \frac{Mv}{t} \times \frac{vt}{2} = \frac{Mv^2}{2}.$$

This result at once reminds us of the term "*vis viva*," literally meaning living force, and shows us that the work expended in giving motion to any body and stored up in it is one-half the *vis viva*, which can be separated into the two terms (Mv) and $\left(\frac{v}{2}\right)$, the first being the momentum and the second the space through which the momentum will act in one second.

Matter has the power of absorbing and storing up work while being given motion, and gives out work when its motion is retarded. A body in motion and not acted on by any force, will move in a straight line and with a constant velocity. If this body is acted upon by a resistant force, it will not stop until all of the work stored in it $\left(M\frac{v^2}{2}\right)$ is given out.

These phenomena are due to the inertia of matter, meaning by inertia the inability of dead matter to change its position or motion of its own accord, and the passive resistance which it offers to all action upon it. "Action and reaction are ever equal, simultaneous and opposite," but in a free body the reaction is always yielding to the

action of a force in the form of motion of the body in the direction of the acting force, and the matter contained in the body acted upon stores up the combined force and motion as work.

The moment of inertia of a rotating body is the statical moment of the momentum of the body and is equal to the sum of the moments of the momenta of its infinitesimal elements; it is the moment of that constant force with which a rotating body would resist being brought to rest in one second; or, being at rest, would resist having impressed upon it a certain angular velocity (w) in one second. Thus each elementary particle (m) would have a momentum (mv), and a lever arm (r) and its moment of inertia would be mvr; but since $v = rw$ we have $mvr = mr^2w$ as the moment of inertia of each particle and Σmr^2w as the moment of inertia of the whole body.

The definitions of and formulæ for the moment of inertia given in many text-books neglect the angular velocity and the acceleration of gravity (g) and are therefore only comparative measures of the moment of inertia, and do not give a clear conception of what the moment of inertia really is.

If it is desired to know the amount of work done in giving the mass m the velocity v in one second, we have at once the expression $(mv)\left(\frac{v}{2}\right)$ for the work $= \frac{1}{2}mv^2 = mr^2\frac{w^2}{2}$.

A more complete and misleading misnomer than moment of inertia would be hard to imagine if the original inventor of the term meant by it what he is usually assumed to mean, viz., "the work lost or gained whilst the body is experiencing a change in the square of its angular velocity equal to unity," or "the weight of a body which, if concentrated at the distance unity from the axis of rotation, would require the same work to produce a given increase of angular velocity which the actual body requires."

The moment of flexure used in discussions of the elasticity and strength of materials is an analogous expression to the moment of inertia, the modulus of elasticity taking the place of the angular velocity; it is a statical measure but mentioned here because of its similarity to the moment of inertia.

The radius of oscillation of a rotating body is equal to its moment of inertia divided by its momentum; it is the mean radius or lever arm of the momentum: the extremity of this radius is the centre of oscillation or, as it is sometimes called, the centre of percussion of

a suspended body, because if the body be struck at this point no shock will be communicated to its axis; it is the point where statical equilibrium occurs between the momentum of that part of the body between it and the axis of rotation and the momentum of that part of the body outside (away from the axis) of the centre of percussion.

Since the angular velocity is a common factor of the moment of inertia and the momentum of a body, it can be and usually is neglected and the radius of oscillation is found by dividing the measure of the moment of inertia of any body by its statical moment.

The centre of pressure is an analogous term to the centre of oscillation and is found in a similar manner. It is a statical term and its analogy is due to the fact that the pressure of water increases with the depth from the surface in the same ratio that momentum increases with the distance from the axis of rotation, or the elastic resistance of materials increases with the distance from the neutral surface of a beam.

The square of the radius of gyration of a rotating body is equal to its moment of inertia divided by the momentum of the mass supposed to be concentrated at a distance equal to unity from its axis, if the angular velocity which is a factor of both terms be neglected, the square of the radius of gyration can be deduced as usually explained in the books by dividing the measure of the moment of inertia (Σmr^2) by the mass (M) of the body, the square of the radius of gyration (k^2) is the mean of the squares of the radii of the infinitesimal elements of any rotating body. The centre of gyration of any body is that point at which, if its mass were supposed concentrating and revolving with the given angular velocity about the axis, the moment of the momentum of this concentrated body (MVk) would equal the sum of the moments of the momenta of its infinitesimal elements (Σmr^2w) while actually rotating around its axis. Since $k^2 = \frac{\Sigma mr^2w}{\Sigma mw}$ we see that it is the ratio between the actual moment of inertia and the moment of inertia of the mass assumed concentrated at a distance unity from the axis, the centre of gyration has no actual physical existence as has the centre of oscillation, but it serves a convenient purpose in mathematical investigation. From the definition of the radius of oscillation it will at once be seen that it can be deduced by dividing the square of the radius of gyration by the distance of the centre of gravity of any body from its axis of oscillation.

Force is actually exhibited and its action felt only when there is a change in the *velocity* with which a mass is moving. Centrifugal force, which is the intensity with which any body moving in the arc of a circle resists being drawn towards its centre, is a good example of this.

Centrifugal force equals the momentum of a mass multiplied by its angular velocity (Mvw).

Generally we can say: In any change of velocity, the intensity of the acting force is measured by the mass multiplied by the velocity which would have been generated in one second had the force remained constant.

The theorem of virtual velocities to which frequent reference is made in works on mechanics, expresses in the form of an equation the equality of the elementary quantities of power being transmitted by any mechanism. Thus, suppose the work actuating any machine to be Fs and the work being done by the machine to be F_1s_1, then letting dt be the differential of the time, we have

$$F\frac{ds}{dt} = F_1\frac{ds}{dt} \text{ or } Fv = F_1v_1.$$

This theorem may also be interpreted by saying that the power of all parts of a continuous train of mechanism at any instant is the same, frictional losses being neglected.

The question at once arises: What is a machine? To which we would answer that it is "an assemblage of resistant parts," for the purpose of conveying *work* from one point to another by means of predetermined motions.

Many of the laws relating to machinery are incorrectly phrased, and we repeat them in a corrected form:

No machine can give out more work than is put into it.

Losses of work occur in all machinery because of friction.

Lost work due to friction usually disappears as heat.

Whatever is gained in speed is lost in force in all machines.

The work done by every member of a continuous train of mechanism is the same for one cycle.

Perpetual motion, *i. e.*, the creation of work, is not possible.

These laws cannot be dispensed with nor in many cases be deduced one from the other; they are many of them mechanical axioms, and are for the most part the result of experience and of a vast number of experiments never recorded and discussions long since forgotten.

Phila., Oct. 24, 1881.

THE SCIENTIFC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING.

By Prof. W. Grylls Adams, F.R.S.

A series of "Cantor Lectures" delivered before the Society of Arts, London, 1881.

(Continued from page 294.)

The fundamental principles which underlie all magneto-electric machines are the four great principles discovered by Oersted, in 1819, by Ampère and Arago in 1820, and by Faraday in 1831. In 1819, Oersted discovered the action of a current of electricity on a magnet. In 1820, Ampère discovered the action of magnets and currents on currents of electricity in their neighborhood, and in the same year Arago showed that currents of electricity produced magnetization, thus laying the foundations of electro-magnetism; and, in 1831, Faraday showed that induced currents were produced by the motion of magnets. All machines for the conversion of work into electricity are founded on Faraday's great discovery of the induced current, derived from the relative motion of a magnet and a coil of wire.

Magneto-electric machines are divided into two great classes, according as they furnish continuous currents or alternate currents. All such machines are alternate, as far as regards the currents in the coil; but these currents are made to flow always in the same direction in the external circuit, in continuous current machines, by means of a commutator which reverses the contact at every half-turn.

The machines of Pixii, in 1832, followed by those of Saxton and Clarke, were the first continuous current machines. From these we may pass to Wheatstone's introduction, in 1845, of electro-magnets in place of permanent magnets, to produce the magnetic field. In 1854, Messrs. Werner, Siemens and Halske introduced the Siemens armature, in which the coil is wound longitudinally in a groove. The strength of the continuous current depends on the velocity of rotation, on the length of the wire and on the power of the magnetic field formed by the magnets.

It is remarkable that, in 1854, Hjorth originated an idea which was some thirteen years in advance of his time; he patented an improved magneto-electric battery, in which the currents induced in

the revolving armature pass round the electro-magnets and increase their magnetism, and so increase the induced currents at compound interest rate. This was the celebrated principle afterwards re-discovered by Siemens and by Wheatstone simultaneously in 1867, which has formed the basis of all dynamo-electric machines and which, for equal power, are cheaper and more compact than all other magneto-electric machines.

Dynamo-Electric Machines.

In February, 1867, Dr. Siemens and Sir Charles Wheatstone, on the same evening, presented to the Royal Society their two papers, "On the Augmentation of the Power of a Magnet by the Reaction Thereon of Currents Induced by the Magnet Itself." According to the principle then put forward by Dr. Siemens, the rotating armature, the electro-magnet and the external resistance, such as an electric lamp, are joined up so as to form one simple circuit. A small amount of magnetism is communicated to the electro-magnet, so that, on rotating the coil, a current is induced alternately in opposite directions, and after being reduced to the same direction by a commutator, this current passes through the coils of the electro-magnet in such a direction as to make it stronger, so enabling it to react on the armature. Thus the magnet and the armature act and react on one another, strengthening the magnetic field and continually strengthening the induced currents. Sir Charles Wheatstone put forward the same principle, and called attention to the fact that at the first instant of completing the combined circuit the effects are stronger than they are permanently. The principle of these dynamo-electric machines is clearly shown in the Figure 1, where N and S represent the poles of the electro-magnet enclosing the revolving armature, the external resistance in the circuit being represented by an electric lamp. Sir Charles Wheatstone also pointed out that a very remarkable increase of all the effects is observed when a shunt is employed to divert a great portion of the current from the electro-magnet. By that means, four inches of platinum wire, ·0067 in. diameter, was

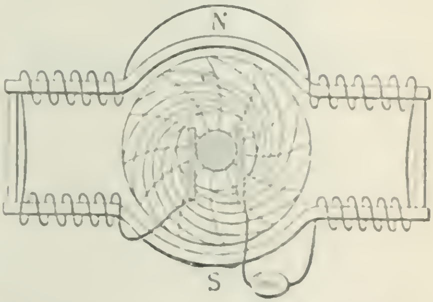


Fig. 1.—Dynamo-Electric Machine.

made to glow. A certain resistance in the shunt was found to be necessary to produce the best effects, so as neither to weaken the magnetism too much nor to give the current too much work to do in heating a high resistance.

In addition to this, Sir Charles Wheatstone showed that the effects above described are far inferior to the effects obtained by placing the work to be done in the shunt circuit. Thus seven inches of wire were made to glow in the shunt, when only four inches of the same wire would glow in the original circuit. There is thus a double advantage, for there is no loss of resistance by introducing the shunt, the resistance of the shunt being the resistance on which the useful work is done.

This improvement, suggested by Sir Charles Wheatstone in 1854, and now being adopted by Dr. C. W. Siemens in his latest dynamo-electric machines, is very well shown in Figure 2, for which, as well as for the other figures illustrating this lecture, I am indebted to the kindness of the Director of *La Lumière Electrique.*

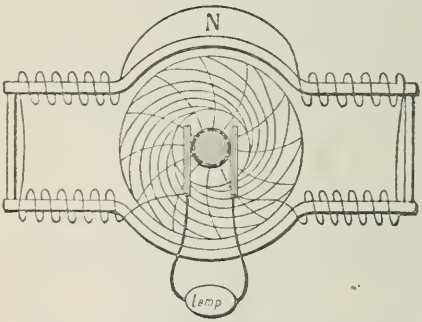


Fig. 2.—Dynamo-Electric Machine.

Wheatstone also showed that the effects are much less influenced by a resistance in the electro-magnet branch than in either of the other branches. Thus, with about four inches of glowing platinum wire in circuit, the addition of about five inches of platinum wire in the armature branch, or in the shunt, produced a $\frac{3}{4}$-inch glow, whilst four feet of the same wire was required in the electro-magnet branch to reduce the glow to three-fourths of an inch. Dr. Siemens has shown that, for the greatest efficiency, the resistance of the rotating coil must be small, but the resistance of the electro-magnet may be increased, and that in both cases the wires should not be small, but of considerable diameter.

The Gramme Ring.

In the Gramme armature, coils of wire are wound in sections, all in the same direction, round a ring; and each section, when a current is flowing through it, may be regarded as an electro-magnet. The

similar poles of all these sectional electro-magnets will point in the same direction round the ring. Consider only one of these electro-magnets with its north pole directed towards the south pole of another magnet, it will be attracted towards it, and with greater and greater force the nearer it approaches; on passing the south pole, its own south pole will be presented to the south pole of the fixed magnet which it has just passed, and its motion will be continued in the same direction.

Now, suppose no current to be flowing in the ring, then, on applying force to produce the same motion as before, the induced current will be in the opposite direction, *i. e.*, as the coil revolves towards the south pole and past it, the induced current in the coil is round the ring, as we look at it from behind, in the direction opposite to the motion of the hands of a watch.

With right-handed winding of the wire on the ring like a cork-screw, the current is coming towards the observer, or in the opposite direction to the motion of the ring on the side nearest the south pole.

If we consider a section of the ring as it approaches and goes away from the north pole of the magnet, the induced current in the coil, as the observer looks at it from behind, will be in the same direction as the hands of a watch move, so that the current will be away from the observer, or in the same direction as the motion of the ring on the side nearest the north pole. Hence, currents flow opposite ways round the ring, in the two halves of the ring, and meet at points equidistant from the two poles.

Dynamo-Electric Machines.

When the armature of a dynamo machine is turned, the amount of work which is produced by means of it is proportional to the number of turns of the armature per minute. If the same current passes through the electro-magnet and the armature, then the current in one acting on the current in the other will attract or repel it with a force proportional to the product of two currents, *i. e.*, to the square of the current The action between the currents is increased four-fold when the current in each is doubled. Hence, when such a machine is used as a generator of an electric current, the external work which can be done by that current in the electric arc or elsewhere is proportional to the square of the current.

Use of Separate Exciting Machine.

In all dynamo-electric machines, where the same current passes round the magnet and the armature, any disturbance in the resistance of the outer circuit, in the electric arc for instance, at once alters the current, and this alters the strength of the magnetic field, which again produces a further disturbance in the current, so that any disturbance is intensified, just as the permanent magnetism of the iron core is intensified by the action of the current in the machine on itself.

To obtain greater regularity, Wilde, in 1863, proposed to employ a separate continuous current machine to give a permanent magnetic field and to revolve the armature of the second machine between the poles of the magnet which is excited by the first machine. In order to find the yield or effective work of these machines, and their efficiency for electric lighting or for other purposes, we have seen, from the laws of Ohm and Joule, that measurements of current and of the work done by the current must be made.

In the last lecture I indicated four methods of making such measurements, viz.:

1. The galvanometer method.

2. The heat method, *i. e.*, by the change of temperature produced by the current in a wire of given resistance.

3. The electrometer or potentiometer method.

4. The electro-dynamometer method, *i. e.*, by the attraction between different parts of the same current.

Efficiency of Magnetic and Magneto-Electric Machines.

If we take a battery in a closed circuit, we know, from the laws of Faraday, that the amount of current produced is directly proportional to the weights of the chemical elements decomposed in each of the cells of the battery, the quantity of zinc dissolved in the battery being a measure of the quantity of current which has passed. According to the laws of transformation of energy, the work done in the chemical actions is here equivalent to the work done in heating the circuit. We may express the energy or the work done by electric currents in the same way as we express the energy of a head of water by the pressure multiplied by quantity of water. The electro-motive force corresponds to pressure, and the current flowing to the quantity of

water, so that the work done by the current is the product of electro-motive force by the quantity of electricity.

The work which can be done in the circuit is E_0C_0, where E_0 is the electro-motive force of the battery, and this is spent in heating the resistance. Hence, $E_0C_0=C_0^2R$, where C_0 is the current produced through a resistance R. Now, if any portion of the circuit, carrying a current, be set in motion, under the action of exterior magnetic forces, or under the influence of the mutual reactions of the currents in the fixed and movable parts, then the equivalent to the chemical actions in the battery will be spent in producing the heating of the conductor, and partly in doing the work of the electro-dynamic or magnetic forces. Representing the work done in producing motion by K, and the current in this case by C, we get $E_0C=C^2R+K$. The work K is equivalent to the external work done by the current C. Now, this external work K gives rise to an opposing electro-motive force in the induced circuit, and the energy of this induced current is $EC=K$; or $E=\frac{K}{C}$ is the electro-motive force due to induction.

The efficiency of an induction machine, when used as a motor, is the ratio of the work K to the total work $K+C^2R$, *i. e.*, the ratio of the electro-motive force of induction to the total electro-motive force of the battery. The efficiency $\rho=\frac{E}{E_0}=\frac{K}{K+C^2R}=\frac{1}{1+\frac{C^2R}{K}}$. The effective work $K=EC$ and $CR=E_0-E$. Therefore, $K=\frac{E.(E_0-E)}{R}$.

If, then, a battery of electro-motive force E_0 be employed, and an induction machine be employed as a motor, then the effective work depends on the product of two quantities, one of which increases as fast as the other diminishes.

Now, if one quantity increases as fast as another diminishes, their product is greatest when the two are equal. Hence such a machine is most effective when $E=E_0-E$, *i. e.*, when $E=\frac{1}{2}E_0$, so that $K=C^2R$ and $\rho=\frac{1}{2}$.

Half the work of the battery is then spent in heating the circuit and the other half in doing the external work. This corresponds to the case where the strength of the battery current is diminished by one-half through the effects of induction. If the same machine be

employed to produce a current of electricity by applying external work to it to turn it, then the energy of the induced current is equal to the work done by the currents during the motion, or $K=EC$. If K is greater than EC at first, then the machine will go faster and faster, and E and C will increase until the product becomes equal to K, when the motion will remain steady. Hence, such a machine should give induction currents of the greatest efficiency when used as a motor.

The above conclusions have been arrived at by considering the attractive and repulsive forces between different parts of a battery current, where the electro-motive force of the battery does not change.

Magnetic Machines.

The same reasoning will apply to the case where a permanent magnet is used in place of the original battery current, a closed circuit being moved in the magnetic field, or a magnet moved in the neighborhood of a fixed coil. In fact, the theory of Ampère requires that a small closed current should be equivalent to a magnetic molecule, and so a collection of equal small closed currents, all going the same way and occupying a given area, is equivalent to a collection of equal magnetic molecules with their poles in the same direction, *i. e.*, to a magnet. But a collection of equal small closed currents, all going the same way and occupying a given area, is equivalent to a current around that area.

We may readily express the efficiency of magneto-electric machines by a formula. Taking k to be the effective work done by a unit current in one turn of the armature, and n the number of turns per minute, and C the strength of the current, the total effective work is C^2nk, and the work done in overcoming the resistance, R, of the circuit is $R^2E\theta$; hence, by Joule's and Ohm's laws, if C be the electromotive force, $EC=C^2nk+C^2R$.

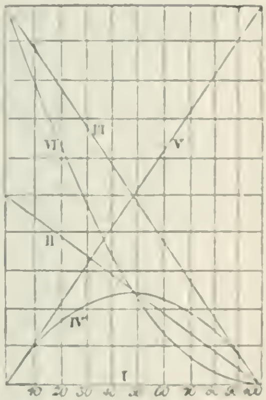
Hence, the efficiency $= \frac{C^2nk}{C^2nk+C^2R} = \frac{1}{1+\frac{R}{nk}}$ Thus, for a given resistance, the greater the number of turns of a machine, the greater is the efficiency, and the higher the resistance, the less the efficiency. This formula shows that machines and lamps of high resistance can only be efficient when the machines are revolving at a high speed.

We may represent the efficiency of batteries or magnetic machines which are employed to drive an electro-motor, such as a dynamo-elec-

tric machine, by a diagram from *La Lumière Electrique*, in which the electro-motive force of the battery or exciting machine is measured on a horizontal line, and either the current or the energy expended is represented by vertical lines. The total electrical energy, represented by a straight line (III), is the sum of the effective work (IV), and the loss of energy by heating, etc. (VI).

It will be seen that when the efficiency is $\frac{1}{2}$, the greatest amount of effective work is produced, and this amount of work is one-half the total electrical energy. Since the electro-motive force is proportional to the number of revolutions a minute, this diagram gives the effective work and efficiency at different speeds.

Fig. 3.



I. Electro-motive force.
II. Strength of current.
III. Energy converted into electricity.
IV. Effective work in the external circuit.
V. The efficiency.
VI. Energy not converted into useful work.

In the case where the fixed and the movable parts of the machine are electro-magnets, and the same current passes round both, then we have—

(1.) The action of the currents on one another.

(2.) The action of the fixed magnetic core on the movable coil, and of the movable magnetic core on the fixed coil.

(3.) The action of the two magnetic cores on one another. Hence, the value of k is of the form of $a+bm+cm^2$, where a, b, c are constants, and m depends on the magnetic properties of the cores.

Each of these actions arises from the influence of two equal currents on one another, and, therefore, will be proportional to the square of the current and to the number of turns of the coil in a minute, so that K is proportional to $nC^2\,(a+bm+cm^2)$ and the efficiency

$$\rho=\frac{1}{1+c_2R}=\frac{1}{1+\dfrac{R}{n\,(a+bm+cm^2)}}.$$

The most important part of the action in such machines depends on the action of the two magnetic cores one upon another. The reactions between the fixed and the movable cores produces great disturbance, in consequence of the cores not taking up their full magnetism

instantly, but requiring time for their full magnetization. In consequence of this, in all dynamo-electric machines, it is necessary to allow the rotation to go on, through an angle determined by the retardation of the magnetism of the cores, before taking away the current from the machine to do external work; hence, in order to get the greatest current, the springs for making contact must in such cases be shifted round in the direction in which the rotation is taking place. Faraday attributed this retardation to the time required to develop the molecular currents in the molecules of the magnet.

Alternate-Current Magneto Machines.

In alternate-current machines, there is no commutator for making the current continuous; but the currents from the coil are collected and sent through the external resistance in opposite directions for every half-turn of the armature. The earliest of these was the "Alliance" magneto-electric machine, which has been adopted by the French government for lighthouse illumination. Holmes converted this into a continuous current machine, and was the first to produce, in 1858, the electric light on a grand scale for lighthouse illumination. He afterwards removed the commutator and again converted it into an alternate-current machine.

The four methods of placing the coils on the revolving wheel, in machines for electric lighting, which have been employed in lighthouses, have been summed up by Mr. Douglass:

1. In Holmes' magneto-electric machine, the bobbins are arranged transversely, with their axes parallel to the axis of rotation around the circumference at the wheel.

2. In Siemens' machines, the wires are lengthwise and revolve about the long axis of the piece on which they are wound.

3. In Gramme's machines, the wires form a helix around a ring.

4. In the De Meritens machines, the wires are wound as in the Gramme, but are divided into separate parts which are insulated from one another and, passing in succession in front of opposite poles of magnets, give off alternate currents.

The Siemens Alternate-Current Machine.

A central disk carrying bobbins is set at right angles to a shaft and revolves between two sets of electro-magnets ranged in circles on each side of the disk, having their axes parallel to the shaft. The bobbins

have no iron cores, and so the heating caused by magnetization and demagnetization of the iron is avoided. The electro-magnets are excited by a small Siemens continuous-current machine.

This is similar to Wilde's dynamo-electric machine, which he produced in 1866, except that in the coils on his cast-iron disk Wilde placed soft iron cores and arranged them so as to form eight groups of four each. The current from one of these groups excites the electro-magnets, whilst the other seven groups give out the current for external use. Among the more recent alternate-current machines, arranged so as to be excited by a separate continuous-current machine, there is one which has lately been invented by M. Gramme, in which the bobbin of the continuous-current machine, which excites the magnets, is placed on the same axis with the rotating armature which gave the alternate currents, so that the two turn with the same velocity and the combined machines run at the same rate. This is much simpler than having two machines, and it is called a self-acting machine.

From principles which I have already explained, the greatest amount of effective work or yield obtained from an alternate-current machine, driven by a separate exciter, is not more than 50 per cent. of the electrical work given out by the first machine.

Result of MM. Mascart and Jamin's Experiments.

M. Jamin has found that, in the Alliance machine for electric light and giving alternate currents, the strength of current can be calculated by Ohm's law, considering the electro-motive force as proportional to the velocity, but replacing the resistance of the bobbins by a resistance about eight times as great. The resistance to be added is proportional to the velocity of the machine.

All theoretical determinations of the efficiency of machines are complicated by the retardation of magnetization of the magnets, which necessitates a change of position of the commutator in the direction of the rotation of the armature. If this change were not made, then it would be possible, with a bobbin in which the wire resistance was high, to get currents in opposite directions when the coil is rotated at different rates. For slow rotation, the galvanometer needle is deviated to one side; on increasing the velocity of rotation, the current is increased, at last reaches a maximum, and beyond that diminishes rapidly to nothing and becomes negative; as the velocity

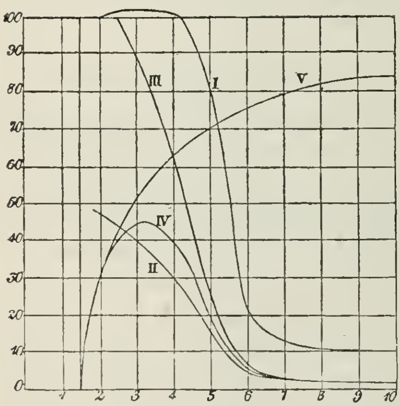
is still increased the current reaches its greatest negative value, and then increases again, and may have several such fluctuations.

The efficiency of the Siemens dynamo-electric machines has been examined experimentally by Dr. C. W. Siemens, who has communicated the results of his investigations to the Royal Society, and by Dr. Hopkinson, whose results are published in the *Transactions of the Institution of Mechanical Engineers;* also M. Hospitalier and Messrs. Auerbach and Meyer have experimented on Gramme machines, and MM. Mascart and Angot have considered the subject, both theoretically and practically, in some excellent papers which have been given in the *Journal de Physique.*

Taking the experiments of M. Hospitalier, as given in *La Lumière Electrique,* we get the results, as shown in the diagram:

	Ohms.
The total resistance of the machine before the experiment was	1·185
" " of the heated bobbin	·75
" " of the heated electro-magnet,	·72
Total,	1·47

Fig. 4.



I. Electromotive force.
II. Strength of current.
III. Energy converted into electricity.
IV. Useful work in the external circuit.
V. Efficiency.

The resistances are laid down on the horizontal line to the scale of 1 c.m. per ohm.

The scale of electro-motive force (I) is 1 c.m. for 10 volts.

The scale for currents (II) is 1 c.m. for 10 webers.

The scale for work (III and IV) is 1 c.m. for 20 kilogrammetres.

Curve V represents the efficiency, *i. e.*, the ratio of the effective work in the outer circuit to the total work converted into electricity.

This curve expresses the ratio of the external work to the total work produced, and approaches the value unity as the resistance is increased; but for high resistances very little work is produced. From the curves it would appear that this Gramme machine does most effective work in the outer current,

without greatly heating the internal circuit, when the total resistance is about 4 ohms, *i. e.*, when the external resistance is about twice as great as the internal resistance. For less resistances the machine is greatly heated from the amount of internal work consumed, and for higher resistances the work converted into electricity becomes very small. The following numbers give the results of one experiment with the dynamo machine:

Number of turns a minute, . .	1000
External resistance, . . .	2·7 ohms
Current, ...	25·5 webers
Electro-motive power, . . .	107 volts
Total work converted into electricity, .	3·64 h. p.
Effective work in external circuit, . .	2·38 h. p.

The efficiency in this case is 65 per cent. The effective work in the outer circuit is not more than 50 per cent. of the total work done to produce it.

(To be continued.)

Cometary Spectra.—Thollon has presented to the French Academy an account of some comparative observations upon the spectra of the two recent comets. In the first the continuous spectrum predominated, and finally it almost obscured the band spectrum. In the second the three carbon bands were very well defined and very brilliant, but gradually fading upon the side of the violet, and he was not able to observe any slight traces of a continuous spectrum until the night of the 21st of August. The brilliancy of the head and tail varies rapidly with the distance from the sun. In the absence of any precise photometric measurements the variations seemed to be nearly in the inverse ratio of the square of the distance. If this law should hold rigorously it would probably follow that the white light of comets is almost entirely due to reflection of the sunlight. It seems hard to reconcile the slowness with which the brilliancy of the band spectrum varies with the common opinion that the elements of a comet are raised to incandescence by the sun's heat. Comets, like nebulæ, may have some heat and light of their own, independent of that which may come from solar, mechanical and electric action.—*Comptes Rendus.* C.

On the SOURCES of ENERGY in NATURE AVAILABLE to MAN for the Production of MECHANICAL EFFECT.

By Sir William Thomson.

Address delivered before the Physical Science Section of the British Association, 1881.

During the fifty years' life of the British Association, the Advancement of Science for which it has lived and worked so well has not been more marked in any department than in the one which belongs very decidedly to the Mathematical and Physical Section—the science of Energy. The very name energy, though first used in its present sense by Dr. Thomas Young about the beginning of this century, has only come into use practically after the doctrine which defines it had, during the first half of the British Association's life, been raised from a mere formula of mathematical dynamics to the position it now holds of a principle pervading all nature and guiding the investigator in every field of science.

A little article communicated to the Royal Society of Edinburgh, a short time before the commencement of the epoch of energy, under the title "On the Sources Available to Man for the Production of Mechanical Effect,"* contained the following:

"Men can obtain mechanical effect for their own purposes by working mechanically themselves and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air, and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes:

"I. The food of animals.

"II. Natural heat.

"III. Solid matter found in elevated positions.

"IV. The natural motions of water and air.

"V. Natural combustibles (as wood, coal, coal-gas, oils, marsh-gas, diamond, native sulphur, native metals, meteoric iron).

* Read at the Royal Society of Edinburgh on February 2d, 1852. (*Proceedings* of that date.)

"VI. Artificial combustibles (as smelted or electrically-deposited metals, hydrogen, phosphorus).

"In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions:

"*Heat radiated from the sun* (sunlight being included in the term) *is the principal source of mechanical effect available to man.** From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, galvanic engines, windmills, and the sails of ships.

"2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly no doubt from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides.

"3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body—or meteoric—that is, belonging to bodies deposited on the earth from external space. Terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of inorganic native combustibles, are actually used; but the mechanical effect obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes."

Thus we may summarize the natural sources of energy as tides, food, fuel, wind and rain.

Among the practical sources of energy thus exhaustively enumerated, there is only one not derived from sun-heat—that is the tides. Consider it first. I have called it *practical*, because tide-mills exist. But the places where they can work usefully are very rare, and the whole amount of work actually done by them is a drop to the ocean of work done by other motors. A tide of two metres' rise and fall, if

* A general conclusion equivalent to this was published by Sir John Herschel, in 1833. See his *Astronomy*, edit. 1849, § (399).

we imagine it utilized to the utmost by means of ideal water-wheels doing with perfect economy the whole work of filling and emptying a dock-basin in infinitely short times at the moment of high and low water, would give just one metre-ton per square metre of area. This work done four times in the twenty-four hours amounts to $\frac{1}{1620}$ of the work of a horse-power. Parenthetically, in explanation, I may say the French metrical equivalent (to which in all scientific and practical measurements we are irresistibly drawn, notwithstanding a dense barrier of insular prejudice most detrimental to the islanders)—the French metrical equivalent of James Watt's "horse-power" of 550 foot-pounds per second, or 33,000 foot-pounds per minute, or nearly two million foot-pounds per hour, is 75 metre-kilogrammes per second, or $4\frac{1}{2}$ metre-tons per minute, or 270 metre-tons per hour. The French ton of 1000 kilogrammes used in this reckoning is 0·984 of the British ton.

Returning to the question of utilizing tidal energy, we find a dock area of 162,000 square metres (which is little more than 400 metres square) required for 100 horse-power. This, considering the vast costliness of dock construction, is obviously prohibitory of every scheme for economizing tidal energy by means of artificial dock-basins, however near to the ideal perfection might be the realized tide-mill, and however convenient and non-wasteful the accumulator — whether Faure's electric accumulator, or other accumulators of energy hitherto invented, or to be invented, which might be used to store up the energy yielded by the tide-mill during its short harvests about the times of high and low water, and to give it out when wanted at other times of six hours. There may, however, be a dozen places possible in the world where it could be advantageous to build a sea-wall across the mouth of a natural basin or estuary, and to utilize the tidal energy of filling it and emptying it by means of sluices and water-wheels. But if so much could be done, it would in many cases take only a little more to keep the water out altogether, and make fertile land of the whole basin. Thus we are led up to the interesting economical question, whether is forty acres (the British *agricultural* measure for the area of 162,000 square metres) or 100 horse-power more valuable. The annual cost of 100 horse-power night and day, for 365 days of the year, obtained through steam from coals, may be about ten times the rental of forty acres at £2 or £3 per acre. But the value of land is essentially much more than its rental, and the rental of land is apt to be much more

than £2 or £3 per acre in places where 100 horse-power could be taken with advantage from coal through steam. Thus the question remains unsolved, with the possibility that in one place the answer may be *one hundred horse-power*, and in another *forty acres*. But, indeed, the question is hardly worth answering, considering the rarity of the cases, if they exist at all, where embankments for the utilization of tidal energy are practicable.

Turning now to sources of energy derived from sun-heat, let us take the wind first. When we look at the register of British shipping and see 40,000 vessels, of which about 10,000 are steamers and 30,000 sailing ships, and when we think how vast an absolute amount of horse-power is developed by the engines of those steamers, and how considerable a proportion it forms of the whole horse-power taken from coal annually in the whole world at the present time, and when we consider the sailing ships of other nations, which must be reckoned in the account, and throw in the little item of windmills, we find that, even in the present days of steam ascendency, old-fashioned Wind still supplies a large part of all the energy used by man. But however much we may regret the time when Hood's young lady, visiting the fens of Lincolnshire at Christmas, and writing to her dearest friend in London (both sixty years old now if they are alive), describes the delight of sitting in a bower and looking over the wintry plain, not desolate, because "windmills lend revolving animation to the scene," we cannot shut our eyes to the fact of a lamentable decadence of wind-power. Is this decadence permanent, or may we hope that it is only temporary? The subterranean coal-stores of the world are becoming exhausted surely, and not slowly, and the price of coal is upward bound—upward bound on the whole, though no doubt it will have its ups and downs in the future as it has had in the past, and as must be the case in respect to every marketable commodity. When the coal is all burned; or, long before it is all burned, when there is so little of it left and the coal-mines from which that little is to be excavated are so distant and deep and hot that its price to the consumer is greatly higher than at present, it is most probable that wind-mills or wind-motors in some form will again be in the ascendant, and that wind will do man's mechanical work on land at least in proportion comparable to its present doing of work at sea.

Even now it is not utterly chimerical to think of wind superseding coal in some places for a very important part of its present duty—that

of giving light. Indeed, now that we have dynamos and Faure's accumulator, the little want to let the thing be done is cheap windmills. A Faure cell containing 20 kilogrammes of lead and minium, charged and employed to excite incandescent vacuum-lamps, has a light-giving capacity of 60-candle hours (I have found considerably more in experiments made by myself; but I take 60 as a safe estimate). The charging may be done uninjuriously, and with good dynamical economy, in any time from six hours to twelve or more. The drawing off of the charge for use may be done safely, but somewhat wastefully, in two hours, and very economically in any time of from five hours to a week or more. Calms do not last often longer than three or four days at a time. Suppose then, that a five days' storage-capacity suffices (there may be a little steam-engine ready to set to work at any time after a four days' calm, or the user of the light may have a few candles or oil-lamps in reserve, and be satisfied with them when the wind fails for more than five days). One of the 20-kilogramme cells charged when the windmill works for five or six hours at any time, and left with its 60-candle hours' capacity to be used six hours a day for five days, gives a 2-candle light. Thus thirty-two such accumulator cells so used would give as much light as four burners of London 16-candle gas. The probable cost of dynamo and accumulator does not seem fatal to the plan, if the windmill could be had for something comparable with the prime cost of a steam-engine capable of working at the same horse-power as the windmill when in good action. But windmills as hitherto made are very costly machines; and it does not seem probable that, without inventions not yet made, wind can be economically used to give light in any considerable class of cases, or to put energy into store for work of other kinds.

Consider, lastly, rain-power. When it is to be had in places where power is wanted for mills and factories of any kind, water-power is thoroughly appreciated. From time immemorial, water-motors have been made in large variety for utilizing rain-power in the various conditions in which it is presented, whether in rapidly-flowing rivers, in natural waterfalls, or stored at heights in natural lakes or artificial reservoirs. Improvements and fresh inventions of machines of this class still go on; and some of the finest principles of mathematical hydrodynamics have, in the lifetime of the British Association, and, to a considerable degree, with its assistance, been put in requisition for

perfecting the theory of hydraulic mechanism and extending its practical applications.

A first question occurs: Are we necessarily limited to such natural sources of water-power as are supplied by rain falling on hill-country, or may we look to the collection of rain-water in tanks placed artificially at sufficient heights over flat country to supply motive-power economically by driving water-wheels? To answer it: Suppose a height of 100 metres, which is very large for any practicable building, or for columns erected to support tanks; and suppose the annual rainfall to be three quarters of a metre (30 inches). The annual yield of energy would be 75 metre-tons per square metre of the tank. Now one horse-power for 365 times 24 hours is 236,500 foot-tons; and therefore (dividing this by 75) we find 3153 square metres as the area of our supposed tank required for a continuous supply of one horse-power. The prime cost of such a structure, not to speak of the value of the land which it would cover, is utterly prohibitory of any such plan for utilizing the motive power of rain. We may or may not look forward hopefully to the time when windmills will again "lend revolving animation" to a dull flat country; but we certainly need not be afraid that the scene will be marred by forests of iron columns taking the place of natural trees, and gigantic tanks overshadowing the fields and blackening the horizon.

To use rain-power economically on any considerable scale we must look to the natural drainage of hill country, and take the water where we find it either actually falling or stored up and ready to fall when a short artificial channel or pipe can be provided for it at moderate cost. The expense of aqueducts, or of underground water-pipes, to carry water to any great distance—any distance of more than a few miles or a few hundred yards—is much too great for economy when the yield to be provided for is *power;* and such works can only be undertaken when the *water itself* is what is wanted. Incidentally, in connection with the water supply of towns, some part of the energy due to the head at which it is supplied may be used for power. There are, however, but few cases (I know of none except Greenock) in which the energy to spare over and above that devoted to bringing the water to where it is wanted, and causing it to flow fast enough for convenience at every opened tap in every house or factory, is enough to make it worth while to make arrangements for letting the water-power be used without wasting the water-substance. The cases in which water-

power is taken from a town supply are generally very small, such as working the bellows of an organ or "hair brushing by machinery," and involve simply throwing away the used water. The cost of energy thus obtained must be something enormous in proportion to the actual quantity of the energy, and it is only the smallness of the quantity that allows the convenience of having it when wanted at any moment, to be so dearly bought.

For anything of great work by rain-power, the water-wheels must be in the place where the water supply with natural fall is found. Such places are generally far from great towns, and the time is not yet come when great towns grow by natural selection beside waterfalls for power, as they grow beside navigable rivers for shipping. Thus hitherto the use of water-power has been confined chiefly to isolated factories which can be conveniently placed and economically worked in the neighborhood of natural waterfalls. But the splendid suggestion made about three years ago by M. Siemens in his presidential address to the Institution of Mechanical Engineers, that the power of Niagara might be utilized by transmitting it electrically to great distances, has given quite a fresh departure for design in respect to economy of rain-power. From the time of Joule's experimental electro-magnetic engines, developing 90 per cent. of the energy of a voltaic battery in the form of weights raised; and the theory of the electro-magnetic transmission of energy completed thirty years ago on the foundation afforded by the train of experimental and theoretical investigations, by which he established his dynamical equivalent of heat in mechanical, electric, electro-chemical, chemical, electro-magnetic and thermo-electric phenomena, it had been known that potential energy from any available source can be transmitted electro-magnetically by means of an electric current through a wire, and directed to raise weights at a distance, with unlimitedly perfect economy. The first large-scale practical application of electro-magnetic machines was proposed by Holmes in 1854, to produce the electric light for lighthouses, and persevered in by him till he proved the availability of his machine to the satisfaction of the Trinity House and the delight of Faraday, in trials at Blackwall in April, 1857, and it was applied to light the South Foreland lighthouse on December 8, 1858. This gave the impulse to invention; by which the electro-magnetic machine has been brought from the physical laboratory into the province of engineering, and has sent

back to the realm of pure science a beautiful discovery—that of the fundamental principle of the dynamo, made triply and independently, and as nearly as may be simultaneously, in 1867, by Dr. Werner Siemens, Mr. S. A. Varley and Sir Charles Wheatstone; a discovery which constitutes an electro-magnetic analogue to the fundamental electrostatic principle of Nicholson's revolving doubler, resuscitated by Mr. C. F. Varley in his instrument "for generating electricity," patented in 1860, and by Holtz in his celebrated electric machine, and by myself in my "replenisher" for multiplying and maintaining charges in Leyden jars for heterostatic electrometers, and in the electrifier for the siphon of my recorder for submarine cables.

The dynamos of Gramme and Siemens, invented and made in the course of these fourteen years since the discovery of the fundamental principle, give now a ready means of realizing economically on a large scale, for many important practical applications, the old thermodynamics of Joule in electro-magnetism; and, what particularly concerns us now in connection with my present subject, they make it possible to transmit electro magnetically the work of waterfalls through long insulated conducting wires and use it at distances of fifties or hundreds of miles from the source, with excellent economy—better economy, indeed, in respect to proportion of energy used to energy dissipated, than almost anything known in ordinary mechanics and hydraulics, for distances of hundreds of yards instead of hundreds of miles.

In answer to questions put to me in May, 1879,* by the Parliamentary Committe on Electric Lighting, I gave a formula for calculating the amount of energy transmitted and the amount dissipated by being converted into heat on the way, through an insulated copper conductor of any length, with any given electro-motive force applied to produce the current. Taking Niagara as example, and with the idea of bringing its energy usefully to Montreal, Boston, New York and Philadelphia, I calculated the formula for a distance of 300 British statute miles (which is greater than the distance of any of those four cities from Niagara, and is the radius of a circle covering a large and very important part of the United States and British North America), I found almost to my surprise that, even with so great a distance to be provided for, the conditions are thoroughly practicable

* Printed in the Parliamentary Blue Book Report of the Committee on Electric Lighting, 1879.

with good economy, all aspects of the case carefully considered. The formula itself will be the subject of a technical communication to Section A in the course of the meeting on which we are now entering. I therefore, at present, restrict myself to a slight statement of results:

1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth-connection and the near end of a solid copper wire of half an inch (1·27 centimetres) diameter and 300 statute miles (483 kilometres) length.

2. Let resistance by driven dynamos doing work, or by electric lights, or—as I can now say—by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth-plate there.

3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse-power, losing 5250 (or 20 per cent.) of this by the generation and dissipation of heat through the conductor, and 21,000 horse-power (or 80 per cent. of the whole) on the recipients at the far end.

4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by convection is only about 20° Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 75,000 Daniells) is (Thomson's "Electrostatics and Magnetism," sec. 340) only 18 millimetres, and therefore there is no difficulty about the insulation.

6. The cost of the copper wire, reckoned at 8d. per lb., is £37,000; the interest on which at 5 per cent. is £1900 a year. If 5250 horse-power at the Niagara end costs more than £1900 a year, it would be better economy to put more copper into the conductor; if less, less. I say no more on this point at present, as the economy of copper for electric conduction will be the subject of a special communication to the Section.

I shall only say, in conclusion, that one great difficulty in the way of economizing the electrical transmitting power to great distances (or even to moderate distances of a few kilometres) is now overcome by Faure's splendid invention. High potential—as Siemens, I believe, first pointed out—is the essential for good dynamical economy in the electric transmission of power. But what are we to do

with 80,000 volts when we have them at the civilized end of the wire? Imagine a domestic servant going to dust an electric lamp with 80,000 volts on one of its metals! Nothing above 200 volts ought on any account ever to be admitted into a house or ship, or other place where safeguards against accident cannot be made absolutely and forever trustworthy against all possibility of accident. In an electric workshop, 80,000 volts is no more dangerous than a circular saw. Till I learned Faure's invention I could but think of step-down dynamos, at a main receiving station, to take energy direct from the electric main with its 80,000 volts, and supply it by secondary 200-volt dynamos or 100-volt dynamos, through proper distributing wires, to the houses and factories and shops where it is to be used for electric lighting and sewing machines and lathes and lifts, or whatever other mechanism wants driving power. Now the thing is to be done much more economically, I hope, and certainly with much greater simplicity and regularity, by keeping a Faure battery of 40,000 cells always being charged direct from the electric main, and applying a methodical system of removing sets of 50 and placing them on the town-supply circuits, while other sets of 50 are being regularly introduced into the great battery that is being charged, so as to keep its number always within 50 of the proper number, which would be about 40,000 if the potential at the emitting end of the main is 80,000 volts.

Malleable Iron.—M. Forquignon has published an extensive series of researches upon malleable iron and the re-heating of steel. Among other conclusions he attaches special importance to the following: 1. Malleable iron always contains amorphous graphite; 2. A casting may lose carbon and yet remain brittle if the original quantity of graphite is not increased; 3. A casting may become malleable without losing any sensible portion of its carbon; 4. If silicium is added to manganesian castings they are improved by re-heating; 5. Hydrogen and nitrogen may unite with the carbon of a casting so as to make it malleable without the production of graphite; 6. The breaking load is always more than doubled, sometime more than quadrupled, by annealing. It increases with the duration of the heating, very rapidly at first and then very slowly; 7. Ductility generally increases with the resistance to breaking, but after a certain limit it has a slight tendency to diminish.—*Ann. de Chim. et de Phys.* C.

RADIO-DYNAMICS: ANOTHER VERIFICATION OF PREDICTION.

By Pliny Earle Chase, LL.D.

On the 4th of October, 1878, I presented a communication to the American Philosophical Society,* in which I showed that the position of Watson's first intra-mercurial planet, as computed by Gaillot and Mouchez, represented the third intra-mercurial term of my harmonic series. At the last meeting of the British Association, Prof. Balfour Stewart read a paper in which he gave indications of sun spot disturbances by a planet revolving in 24·011 days and consequently having a semi-axis major of ·163. This confirmation, both of my own prediction † and of the calculations of the French astronomers, is the more interesting because the first confirmation of my series was contained in a communication which was made to the Royal Society, by Messrs. De la Rue, Stewart and Lowry; forty-one days after I had announced the series to the Philosophical Society and published it in the *New York Tribune.* ‡ The accordances are as follows:

1st interior harmonic term,	·267	De la Rue, S. and L.,	·267
3d " " "	·165	Gaillot and Mouchez,	·164
		Stewart,	·163

New Experiments in Harmonic Vibration. — Decharme mixes a little finely powdered minium in water and covers a horizontal plate of glass with it as uniformly as possible. He then lets fall upon this thin layer a drop of the mixture, and there appears a regular figure formed of the minium, arranged in rays and concentric rings, the whole producing very various designs. The figures resemble in many respects the vibratory forms of circular plates, which he has been studying. The three systems of the Chladni plates, the diametral, circular and compound, are usually coexisting, but either one can be made to predominate over the others at the will of the experimenter.— *Comptes Redus.* C.

* Proc. Am. Phil. Soc., xviii, 34–6.

† *Ibid.*, xiii, 238.

‡ *Ibid.*, p. 470.

Radiant Matter.—Father Serpieri and Prof. Riglio have published, in the *Revista Scientifico-Industriale,* some of their observations upon Crookes' apparatus. Ferrini has repeated the experiments and published the following theory: On account of the very great resistance which, as Hittorf has shown, is produced under the influence of a great rarefaction around the cathode, there arises a powerful induction, and the phosphorescence of the walls of the globe indicates their intersection with the lines of force of the electric field, which is most neatly circumscribed where those lines are the most energetic. The gaseous molecules arrange themselves along those lines while transmitting the electricity. The blue pencil, which corresponds to the most intense portion of the electric field, represents a continuous discharge between the cathode and the glass which is in front of it.—*La Lumière Electrique.* C.

Phosphorescent Alumina.—Crookes has experimented with phosphorescent spectra in a nearly perfect vacuum. After operating for a long time upon chemically pure alumina, which had been precipitated from the sulphate, he noticed a curious phenomenon. When the alumina was first enclosed in the vacuum it was of a snowy whiteness, but after having been exposed frequently to the molecular current, which made it phosphorescent, it gradually assumed a rosy tint, and after two years, when it was exposed to the solar light, it showed a trace of the aluminium line. Repeated molecular excitement gradually brings the amorphous powder into a crystalline form. If ammonia is added in great excess to a diluted solution of alum a part of the alumina is precipitated. If the solution is then filtered and boiled, the alumina which was dissolved by the ammoniacal excess is precipitated. Separating it by filtration, igniting it, and submitting it to the action of the molecular current, instead of giving a red light it has a pale green phosphorescence which, when examined with the prism, shows no lines, but simply a concentration of light in the green portion of the spectrum. In studying rubies, of which he examined an enormous number by his apparatus, Crookes had the good fortune to find a single crystal, which, although it presented no difference to the sight, gave a green light under the action of the molecular current. This green light, however, always had a trace of the red line, and if the action of the molecular current was continued for several minutes the green phosphorescence faded and a red tint was produced. —*Ann. de Chim. et de Phys.* C.

Radiant Absorption of Carbonic Acid. — By experiments upon the absorption of radiant heat by gases, E. Lechler finds that a layer of carbonic acid 917 millimetres (37 in.) thick absorbs 90 per cent. of the luminous radiation. The carbonic acid of our atmosphere is therefore sufficient fully to account for the atmospheric absorption of the Sun's rays.—*Ann. d. Physik.* C.

Illumination of Tubes by the Holtz Machine.—The Geissler tubes can be illuminated by the Holtz machine, but the light is weak; in order to give it brilliancy, condensers and frequent interruptions of the current are required; if stratifications are desired it is necessary to interpose resisting media. Most of the experiments with the Crookes tubes can be performed with the Holtz as well as with the induction coil. Some of the experiments, however, which require very strong coils, are not very satisfactory: such, for example, as the luminous cross, the repulsion of two rays of radiant matter, and the incandescence of platinum.—*Les Mondes.* C.

Constitution of Comets. — M. Prazmowski concludes, from observations since 1858 by the spectroscope and polariscope, that comets are formed of a condensed portion, which constitutes the nucleus, surrounded by an incandescent gaseous atmosphere, which contains carbon and reflects the solar light, and of a swarm of disaggregated material which is not controlled by the cometary attraction but moves in obedience to universal attraction. In some comets the polarization of the light is strongly marked, while in others it is almost wholly absent. In the latter case he compares the structure to that of atmospheric clouds.—*Comptes Rendus.* C.

Connection between Refraction and Absorption of Light. —Ketteler has conducted an extensive series of experiments in order to prove his hypothesis that the refraction and absorption of light are due to the same physical laws. In order to strengthen his results, he tested each both by spectral, metric and photometric methods, and thus was able to show a very satisfactory accordance between the refraction and absorption curves. Each class of constants was found to exhibit the same relations to the concentration of the liquids which were experimented upon, and he regards the experiments as conclusively establishing the identity which is assumed in his hypothesis.—*Wiedemann's Annalen.* C.

Book Notices.

Working Drawings and How to Make and Use Them, designed for Industrial, Technical, etc., Schools and Artisans desiring a knowledge of the principles of Pattern and Template making. By Lewis M. Haupt, Professor of Civil Engineering in the University of Pennsylvania, etc. 12mo. Philadelphia: Jos. M. Stoddard & Co. 1881.

Mr. Editor:—Having been absent from the city, I have only just received the August number of the Journal, which contains a review of my recent little book on "Working Drawings," etc., and as your author has manifestly misunderstood my preface or read it hastily, I desire to correct some of the erroneous impressions his review must create, to the prejudice of the work.

He states first generally that "Engineers and master mechanics * * have been amazed that even graduates of technical and scientific schools *generally* come to them with *entirely erroneous methods of thought* * and practice."

If this statement be true, then the system of instruction in our scientific and technical schools generally must be out of gear, from the use of present standard authorities, not the book under review, as that has not yet been introduced, and it is time the schools should know it and change their system; but I have heard no complaint in that direction, and our graduates seem to have no difficulty in making drawings that are readily understood. At all events, no school should teach errors, and if our system be such we hope to be convinced of it that a change may be immediately effected.

In the second paragraph your reviewer states that "The title and preface of this book give the impression that the author has done a good work in presenting a plan by which cadet engineers can be graduated capable of entering the draughting room and being immediately useful," etc.

If such an impression is created by a careful reading of the preface I must confess that I need to study how to write correctly, for I have distinctly stated therein that the book was designed especially for the very lowest grade of scholars capable of comprehending the subject, viz., those in the public schools, and that this book was but the *first* of a *series* and was intended simply as a test of the ability of the

* The italics are my own.

student to comprehend the principles which underlie the *science* of making drawings. Yet notwithstanding this, your reviewer expects to find in this little volume of 55 pages, sold for 60 cents, and the first one of a series, sufficient instruction to give a draughtsman a "technical knowledge of scales, pencils, instruments, inks and colors, manual skill to use them with neatness and dispatch; and with some experience of the best American practice in regard to the making of working drawings, such as the relative arrangement of different views, the avoiding of unnecessary repetitions, the judicious use of sections, coloring and shade lines, and particularly with a knowledge of the best distribution of dimension lines and figures and with a neat and clear style of making them. An examination of the text will fail to confirm this impression. Instead of being the connecting link between theory and practice, it is based entirely on the theory of Descriptive Geometry and is in direct opposition to the practice of American and English engineers."

Concerning these objections I have only to say, 1st, that it is a waste of printer's ink to attempt to describe instruments and the manner of using them in books when the same information has already been repeated so frequently and when a few hours of practical instruction by a competent draughtsman would be worth more than any amount of description; 2d, that it seems unreasonable to expect the whole of a serial work to appear in its first volume and to condemn the remainder unseen; 3d, that I cannot conceive of a working drawing that is *not* "based *entirely* on the theory of Descriptive Geometry" or projections, intersections and developments; and, 4th, that the last objection is only partially true, since but a small percentage of "American and English engineers" use *any other* than the system of projections represented in the work in question.

Your reviewer uses the word engineer in a restricted sense, evidently meaning only mechanical engineers, and forgets that civil engineers, architects and mining engineers, as well as *many mechanical* engineers use precisely the methods given in this work. Also by far the greater number of foreign countries teach and use the same method, and in conforming to it I have simply followed the precedents of the great bulk of instructors and constructors in this and other countries.

Your reviewer's next paragraph conveys the idea that I have set up a claim for originality in introducing the theory of projections as

applied to working drawings, and quotes Davies' old work as a precedent. Now, as to this work, we instructors think it verbose and wearisome, and prefer the more concise, logical and clearer demonstrations of the late distinguished Prof. Church as tending, more than any other method, to develop intellectual conceptions—and this is the method I have followed in this little book. I make no such claim, but simply the adaptation of the subject to public school instruction.

As a man of science becomes skilled in his profession, it is only natural that he should *apparently* ignore first principles and soar higher with great ease, but as to the "best engineers having long since discarded these theories and methods, with their diedral angles, ground lines, projections, traces, etc.," I must beg leave to doubt. The *principles* are there, and must remain in use in every case just as much as the foundations of a house must remain under it, although unseen. Because these principles are exceedingly simple to an expert it does not follow that they do not need elaboration for the beginners, to whom they are entirely new. To them they must be presented in every phase, so that they may be well grounded in principles, as every teacher can testify.

Your reviewer states again that "A subject so simple as the making of a working drawing * * should not be put in the form of problem, theorem, analysis and construction, but should be based on the experience of eminent engineers," etc. But upon what is the experience of an engineer based, if not upon just such a logical course of reasoning? Must he not know, first, that he desires to create something, to accomplish a certain end? This is his *problem or theorem*. Must he not then *analyze* the machine or building, studying carefully the relations, proportions and operations of its several parts? This is the analysis. And, finally, after his conception is clear on the subject, and he has resolved it out in his brain, then, and only then, can he proceed to represent and *construct* it. And this is his construction. This logical proceeding must occur in every well-disciplined mind; and it is the function of the teacher to develop it as rapidly as possible. Hence the use of mathematics, and the reason why good mathematicians are generally excellent logicians and successful engineers.

So far as I can discover from the review before me, the whole weight of the objection to the book under consideration consists in the fact that whilst a very large number of draughtsmen recognize and use the method of *projecting* the face to be drawn upon a plane placed behind

it, a few prefer to make the drawing upon a transparent plane held in front of it. This is the sole difference; and because your critic is accustomed to the latter method he says that "a book, like the one under review, which teaches obsolete methods and incorrect technicalities, must certainly be misleading and injurious to the people whom it was intended to benefit."

As this is a broad and public assertion, apparently sanctioned by so high an authority as the Franklin Institute, I desire to know for the benefit of the future mechanics and engineers who may receive instruction from technical and scientific schools, if there is a *standard* of modern methods in existence where it is accessible to modern instructors, and, if not, I respectfully suggest that it would be eminently proper for your Committee on Science and the Arts to see that this important defect be supplied, and to recommend the abolition of such works as Church's "Descriptive Geometry," Warren's "Projection and Machine Drawing," Mahan's "Industrial Drawing," Binn's "Orthographic Projections," Watson's "Descriptive Geometry," and many others from the scientific schools of the country as tending to teach *errors*, and to mislead and confuse students, and to cause to be prepared a properly recognized and authenticated standard for use by engineers of every description, whether civil, mechanical, mining, architectural or topographical.

Very respectfully yours,

LEWIS M. HAUPT.

ELEMENTARY PROJECTION DRAWING, THEORY AND PRACTICE. By S. Edward Warren, C.E. Fifth edition. Revised, and with a new division on the Elements of Machines. 8vo. New York: John Wiley & Sons. 1880.

If this well-known book were properly revised and printed, so that it would be *studyable*, it would be highly useful. The greater portion of it is printed from old and worn-out plates, and the folding sheets of illustrations are not only extremely unsuitable for reference, but in many cases absolutely indecipherable. A work so filled with "primes" and "seconds" should be at least well printed.

The first division is on Elementary Projections, and, while very thorough, the treatment is as complicated and artificial as the lettering is illegible.

The second section (details of Masonry, Wood and Metal Construc-

tions) is very scant in its treatment of the various forms of timber joints. Thus, we would expect to find the various single, double, open, full and blind mortise and tenon joints clearly laid out, and blind and false doweling, and dovetailed and keyed miters shown. Piston rod packing is scantily treated, and from an ancient point of view.

In the third division (Elementary Shadows and Shading) lies the real merit of the book.

The fourth, on Isometrical and Oblique Projections, is well handled, the chapter on Oblique or Pictorial Projection being specially interesting.

The new fifth division, on Elements of Machines, is disappointing, especially in relation to gearing. For instance, the instructions, page 132, for construction of spur wheels, would be very obscure even if Plate XVII, to which they refer, were decipherable.

The sixth division, on Simple Structures and Machines, is very fair as a review of practice.

Altogether, we cannot help wishing that an author of such well-known ability as Prof. Warren had carefully revised the text, so as to put it within the comprehension of those not his equals in such matters, and that the publisher had been more liberal, not to say exacting, in the matter of illustrations. R. G.

The Figure of the Earth. An Introduction to Geodesy. By Mansfield Merriman, Professor of Civil Engineering in Lehigh University. 12mo. John Wiley & Sons, New York.

This little book of 88 pages has no other pretension than that of being an introduction to a course of study in geodesy, its substance being largely found in some familiar talks on "the size and shape of the earth," delivered before the students of civil engineering at the Lehigh University. Mr. Merriman calls attention to the fact that the science of geodesy would never have existed had we continued in the ancient paths. The history is given of the early attempts for the determining of the size of the earth from the time of Anaximander (—570) down to the present date, as the earliest measurements were made in the *stadia*, the size of which not being known, the comparisons between the ancient and the modern results cannot be made. The earth has been considered as a spheroid, ellipsoid, ovaloid and as a geoid, and arguments, *pro* and *con*, for these shapes, as well as the general manner of determining important properties of the same, have at various times occupied the consideration of savants. L. S. W.

Franklin Institute.

Hall of the Institute, Oct. 19th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 99 members and 32 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that at the last meeting of the Board 21 persons were elected members of the Institute; also that a vacancy existed in the Board, caused by the resignation of Mr. Chabot.

Mr. Graff moved that the regular order of business be suspended, and that we proceed to make nominations, which was carried.

Mr. Graff nominated Mr. W. H. Thorne. There being no other nominations, the Secretary, upon motion of Mr. McKean, was directed to cast the Institute ballot for the gentleman above named, and the President thereupon declared him elected for the unexpired term of Mr. Chabot.

Mr. Lloyd Wiegand described William Nelson Barrow's improvement in moulding balls, shot and shell, the machine and some of its products being exhibited. The purpose of the invention is to produce articles of perfectly circular form and of exact dimensions by casting, and without recourse to grinding or any other finishing process. Two objects are to be served—one to reduce the cost of production, the other to leave the product with its hard outer skin, thus making it more durable. The castings exhibited were said to be accurate within one one-hundredth part of an inch and were balls (used without grinding or dressing) to draw lap-welded tubes over. The causes of imperfect castings are mainly in the imperfection of the sand mould in form and the rapping or jarring of the pattern to disengage it from the sand which renders the mould perceptibly larger. Barrow's moulding machine dispenses with the rapping, the pattern being withdrawn from the sand by mechanism which guides it accurately in its motion from the sand and holds the latter by a plate fitting around the pattern so as to prevent displacement. To avoid the imperfections due to the ordinary method of venting, the plates in Mr. Barrow's machine have

channels or chambers into which the rammer cannot reach. Venting needles pass through these channels, and are withdrawn with the pattern, leaving the sand loose in the channels, though closely compacted around the pattern. The core rests entirely in the lower part or bottom of the mould, is centered therein by its form, and constitutes the cover or cope of the mould, so that the possibility of maladjustment is avoided. The machine as described has been in use for more than a year, and the exhibited castings were taken indiscriminately from stock. The invention can, of course, be applied to the production of projectiles for ordnance.

Johnston's Air Compressor was shown in operation. The object of this invention, the Secretary stated, is to enable air to be compressed in an economical manner, and with less friction and loss of power than has heretofore been done. It consists of a fixed shaft, with a cylindrical casing, inclosed at its ends and journaled upon the shaft, provided within its upper portion with valve chambers, that contain inlet and outlet valves, and extend downward to the upper side of the shaft. Also a fixed partition, which extends between the lower side of the partition and the lower wall of the casing. Water fills the lower half of the casing, which is then caused to oscillate upon the shaft, the movement in each direction being continued until the front walls of the valve chamber are nearly horizontal, and impinge upon the surface of the water.

The front inlet valve in the direction of the movement will be closed, and the outlet valve of the same side opened, as the valve chambers approach the water-line, and air contained between the former and the surface of the water will be forced into the outlet chamber and into the discharge pipe, the pressure being governed by the relative quantity permitted to escape.

While the air is expelled from one side of the casing it is admitted to the opposite side through the inlet chamber, the alternate filling and discharging being caused by the oscillation of the casing.

In consequence of the practical incompressibility of the water, it presents a solid bearing against which the air is compressed by the downward movement of the valve chambers, which perform in this respect the office of a solid piston. The office of the partition is to hold the water stationary and prevent it from oscillating, as would otherwise be the case.

When a high pressure is desired two or more compressors may be placed upon the same axis, the second one taking the compressed air from the first, etc., the degree thus obtained being governed by the number used.

To keep down the temperature of the compressed air a small stream of water may be admitted with the air supply, the surplus being passed out with the air discharge, and afterwards separated therefrom in a receiver.

The casing can be driven by means of a connecting rod, journaled at one end upon a crank-pin, from the head of the casing, while the other end of the connecting rod is journaled upon the crank of the engine, although any other way of giving an oscillating motion may be adopted.

The mechanism can also be used as a pump, or for exhausting gas or air from mines, etc. When used as a pump the cylinder is submerged, or if above the water a suction pipe is connected.

Mr. Johnston, having been called upon, gave a further explanation of his invention. In reply to inquiries of Mr. Nystrom, he said that the water did not get hot, and that no packing was needed, as the rapid oscillations gave no time for the water to pass the loose fitting partition.

Mr. Wiegand said that he had found water packing used to seal pistons on compound pumps, for compressing nitrous oxide, and for compressing gas used in cars of the Pennsylvania Railroad, very efficient.

These pumps are similar to Mr. Johnston's compressor, in that the cylinders move and the pistons are stationary, leaving the water undisturbed. All the packings are continually covered with water, and the pressure for the compression of nitrous oxide reaches as high as 860 to 900 pounds to the square inch, with no injurious heating. Johnston's Universal Shaft Coupler was also exhibited.

Bennor's Automatic Seal Trap, which was shown, has a mercury seal joint and induction and eduction pipes, the latter enlarged so as to hold a considerable body of water. About a pound and a half of mercury is used in the trap. When the mercury is overbalanced by the water it spreads on an inclined platform forming the bottom of the chamber, thus allowing the water and matter flowing from the basin or sink to pass freely through the exit pipe, after which the

mercury returns to its former position and re-establishes the absolute seal. The high specific gravity of the mercury is relied upon to prevent any back pressure of gas from overcoming the seal and also to prevent syphonage and the evaporation that takes place in an ordinary water seal trap when not in use during the summer months.

Kemble's Lubricator for Wheel Hubs, also shown, has an oil-cup made in the form of a threaded tube, holding a supply of oil and intended to be screwed into the carriage-box, thus bringing the oil in contact with the axles.

Dr. Norris exhibited a new spectrum tube sent to him by Queen & Co., so arranged as to permit the observer to look at the spectrum of a gas from end to end, thus increasing the intensity of the light over that shown by the ordinary tubes, which are placed vertically and looked at transversely. The Secretary stated that in a difficult carbon spectrum, 31 lines were shown by the new tubes, all bright, while with the old forms only eight could be distinguished, and it was calculated a hundred times more light, nearly, was obtained.

Several other inventions were exhibited, among them being the following: H. J. Sills' Blotter, a flexible metal pad, with a convenient means of changing the slips of blotting-paper; Archer's chair for the use of physicians, which can be easily changed from one of ordinary height to a reclining lounge or put in any intermediate position; and the Auburndale metallic thermometers which, in several weeks' test at the Institute, kept within half a degree of a standard thermometer through a range in temperature of from thirty to forty degrees; also a section of the conduits used on Market street by the National Underground Electric Company. The necessary trench to be dug is about 4 feet deep by 18 inches wide and the bottom and sides to be used are lined with hydraulic cement. The tubes are made of tinned iron about two inches in diameter and are in convenient lengths, regulated by the size of the sheets of tin soldered together. Bands of tarred paper are wrapped around the tubes to prevent them from touching and to allow the spaces between them to be filled with the insulating material, which is a composition of asphaltum and slag. The tubes are twenty in number—laid in four rows of five each, one above the other—and each tube will hold a number of insulated wires. The work is completed with concrete on the top, filling in of earth, paving, etc. Manholes are constructed at each square for any necessary repairs.

Mr. Robert Grimshaw read a paper on the application of frictional

electricity to the purification of middlings, illustrated by drawings of the "electric purifier" thrown upon the screen. He first described briefly the old processes of milling, in which the middlings, although among the most valuable products, could not be utilized as flour because they could not be cleaned of bran and other impurities, and then described modern milling, in which the object is to get as many middlings as possible. These middlings have to be purified, and for this purpose various kinds of wind separators have been employed and recently the electrical purifier, of which drawings were exhibited. In these machines frictional electricity is used—hard rubber rolls, electrically excited, attracting to their surfaces the fine bran and lighter impurities from the middlings as the latter pass under them. The bran, etc., is then swept from the rolls by cushions, while the purified middlings are graded by passing through the sieves on which they are carried beneath the rolls. One great advantage of the electric over the wind purifiers is that they get rid of the danger of dust explosions, always present in mills that use currents of air for lifting the lighter impurities from the middlings. This apparatus has been in operation in the Atlantic Mills, Brooklyn, for a year, and the proprietors say that it saves them from ten to twenty cents on every barrel of flour.

Mr. Wm. V. McKean offered the following resolution, which was adopted:

"WHEREAS, attention has been called by the Randolph Street Mill fire to the subject of adequate fire escapes on tall buildings, and to real or supposed dangers attending the lighting the mills by electricity; and, whereas, it is desirable that mill-owners should be informed of the best means of preventing such fires and of affording means of escape for their operatives;

"*Resolved*, That the President be authorized to appoint two committees, one to investigate and report upon dangers incident to electric lighting, if any, and the means of overcoming them, and the other to examine and report upon the principles which should govern the erection of fire escapes and lifts or elevators in new buildings and in those now erected."

Mr. McKean said that he particularly desired to have the committee on fire escapes appointed as, although the law required the erection of such escapes, there was at present no guide as to what constituted a safe escape. He was satisfied in his own mind that iron ladders were unsuited to the use of women and children, and

thought it important to have the whole subject carefully considered by builders and practical men, so that property owners might be given a reliable guide as to what they ought to erect.

Mr. Robert Grimshaw seconded the resolution, and suggested that elevator shafts be also considered by the committee on fire escapes, which suggestion was accepted by Mr. McKean and his resolution amended in accordance therewith. Mr. Grimshaw said that the iron ladders would be very likely in winter to prove practically useless. At the best of times, only workmen used to descending ladders could escape by them; but when cold enough to take the skin of the palm out of one's hands, or when covered with ice, most people would prefer to jump from the windows rather than run the risk of descending by them.

The resolution was unanimously adopted, and the President afterwards appointed the following committees:

On Electric Lighting.—Dr. R. E. Rogers, Dr. C. M. Cresson, David Brooks, Alex. E. Outerbridge, Jr., W. W. Griscom, E. Alex. Scott, Prof. E. J. Houston.

On Fire Escapes and Elevators.—Wm. B. Bement, John Baird, R. K. Betts, Frederick Graff, C. H. Banes, Strickland Kneass, Prof. Wm. D. Marks, Henry G. Morris, J. B. Lippincott.

The following memoir of Henry Cartwright, the late Vice President, prepared by Mr. Washington Jones, was read by the Secretary:

"It is with sorrow that I announce to you officially, the death of our late fellow-member and Vice President, Henry Cartwright, which occurred through an accident early in July last. Mr. Cartwright was for many years an active and influential member of the Franklin Institute, one of its managers and, since January, 1880, one of its Vice Presidents.

"In the discharge of the duties imposed by the position to which you had chosen him he was prompt and efficient, and he willingly gave his time and abilities to promote the interests of the Institute. In the deliberations of the Committee on Science and the Arts, of which he was a valued member of long standing, he always bore part, and his judicial mind and his matured judgment, especially upon mechanical subjects, aided in the formation of just conclusions. As a presiding officer, he maintained the dignity of the chair by calm and courteous demeanor and impartiality in ruling. Commencing his career as a machinist, he soon enlarged his sphere of use-

fulness by engaging in the construction of water works to supply Buffalo and other cities, and by the designing and erection of works for the manufacture of illuminating gas. He was also one of the engineering firm which contracted to cut the now famous tunnel through the Hoosac Mountain for the passage of the Boston and Albany Railroad. Upon his retirement from that work, he became Vice President of the American Meter Company and devoted his time and attention to the manufacture of meters and other details required by gas works for distribution. More recently he was appointed Secretary and Treasurer of a large coal mining company in the western part of this State, and it was in its service that he met his death, whilst in the prime of life, in robust health and cheerful anticipations of the future. In all these varied occupations he possessed such excellent business qualifications, energy and tact as to insure success. With his associates in private life he was even-tempered and agreeable, speaking ill of none, but with a kind word for all. They, as well as we, will miss him, for 'He was a man among men.'"

The following resolution was offered by Mr. McKean and unanimously adopted:

"*Resolved*, That the members of the Institute have heard with deep and sincere regret of the death of Henry Cartwright, at the time Vice President of the Institute; and that the memoir presented by Mr. Washington Jones be entered upon the minutes as an expression of the feelings and judgment of the Institute upon the occasion."

The President announced that maps prepared by the Coast Survey and published by the government had been mounted, arranged in order and catalogued, so as to make them available for the use of members and the public. He called attention to the matter that those who might have use for the maps should know that they were in the library and in condition for ready examination.

Mr. Mitchell presented plans of buildings recently erected in Boston by the Mechanics' Institute there, and said that he hoped the Franklin Institute members would consider the practicability of building some such structure in Philadelphia. In this connection, he suggested the desirability of having the central part of the International Exhibition Building preserved and re-erected on a block of ground in a suitable location.

On motion, the Institute adjourned.

ISAAC NORRIS, M.D., *Secretary.*

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REPORT OF THE COMMITTEE ON THE PRECAUTIONS TO BE TAKEN TO OBVIATE THE DANGERS THAT MAY ARISE FROM SYSTEMS OF ELECTRIC LIGHTING.

The committee, to whom was referred the question of "the dangers incident to electric lighting, if any, and the means of overcoming them," respectfully report as follows, viz.:

That from a careful consideration of the evidence submitted they believe that the use of electricity as an illuminant, as now generally employed, is not attended with any dangers, either to person or property, that cannot be obviated by the adoption of the precautions hereinafter set forth.

In order that the reasons which give rise to the necessity for these precautions may be the more thoroughly understood by the general public, for whom they are designed, the committee believe that the following statements of the general principles involved in systems of electric lighting as now practised, may not be amiss.

There are two systems of electric lighting now in general use, viz.: the "incandescence system" and the "arc system."

In the "incandescence system" an electrical current, flowing through a thin wire of platinum, or other difficultly fusible metal, or through

a thin filament of carbon, heats it by incandescence to nearly a white heat, by reason of the resistance it offers to the passage of the currents.

In the "arc system" an electrical current flowing from one rod of hard carbon to another, heats the rods sufficiently to cause a stream of carbon vapor to pass between them. This vapor, heated to intense whiteness by the electricity, forms what is known technically as the "voltaic arc."

In the "incandescence system" it is necessary, in order to prevent the rapid destruction of the wire, or carbon filament, to surround it completely by a glass globe or cover from which all the air has been removed. In this system, therefore, external objects cannot come into contact with the source of light.

In the "arc system" it is not necessary to exclude the atmospheric air from contact with the carbons, or electrodes as they are sometimes called. Arc lights are, therefore, burned in the open air. They are generally much brighter than incandescent lights and are, in practice, usually surrounded by a globe of glass, in order to lessen the intensity of the glare. They can, however, be placed in a globe from which all the air has been removed, since the light produced is not dependent on the burning of the carbon electrodes—that is, on their combustion.

The source of the electricity employed in all systems of lighting is the dynamo-electric machine. This machine produces, more cheaply, a current whose properties are similar to the current obtained from the ordinary voltaic battery.

In the dynamo-electric machine, mechanical energy, derived from a steam engine, water wheel, or any other suitable source, is converted directly into electrical current. The change of mechanical into electrical energy is obtained by the motion, past powerful magnets, of a number of coils of insulated wire wrapped on a core of iron. The core so wrapped is called the armature. The armature moves close to the poles of the magnets already referred to, but does not actually touch them. There is, therefore, no friction in the dynamo-electric machine as in the ordinary electrical machine. The attraction of the magnet poles tends to hold the armature in fixed positions. Mechanical power is required to move it out of such positions, and when so exerted is converted into electrical currents which flow through the coils of wire on the armature.

The current in the armature flows alternately in different directions through the wire wound thereon. It is generally caused to flow con-

stantly in one and the same direction by means of a contrivance called the commutator.

The current so produced flows out of the machine, through a conducting wire, into and through a number of lamps placed at suitable points in the length of the wire, and finally through a continuation of the wire back to the machine.

The path of the current from the machine to the lamp, and out of the lamp back again to the machine, is known technically as an "electric circuit."

Any conductor is said to be placed in an electric circuit, or to form part of an electric circuit, when it is so arranged that the current from the machine or battery causing the electricity can flow through it and back again to the place where the current was produced. That is, anything placed in an electric circuit forms part of the conducting path through which the current is circulating.

Dynamo-electric machines are now made to furnish very powerful currents; currents capable of sustaining fifty or more arc lights in one circuit or line. Such currents require care in their management, and judgment in their introduction into buildings or public places for purposes of illumination.

Fortunately, however, the currents produced by dynamo-electric machines have some properties very different from those produced by the ordinary frictional machine. The former possess little or no power of leaping from one conductor to another across an intervening non-conductor, such for example as air; the latter can, as is well known, readily so pass, often through several feet or more of dry air.

The currents from dynamo-electric machines, or the currents employed in systems of electric lighting, are not, therefore, at all comparable to strokes of lightning, to which they have often been ignorantly likened. Lightning discharges frequently pass through miles of air, but, even in the largest machines, the carbons employed in arc lamps must first be brought into contact and afterwards separated, before the arc is established and the current passes between them. The current will not, in fact, leap through the air. The momentary contact of the two carbons develops sufficient heat to form a cloud of carbon vapor between them, and this cloud, being an electrical conductor, permits the current to pass.

When, however, the cloud of vapor is once established between the two carbons, it will continue to pass until the carbons are sufficiently

and so for any other proportion. In other words, the currents will be consumed to cause the distance between them to become too great to be bridged over by the vapor. While, therefore, the current possesses little or no power of so leaping through air, yet it is necessary that no opportunity be afforded it to form such conducting clouds between it and neighboring conductors. A brief mention of some of the ways in which such clouds may be formed will, therefore, be of practical importance.

If two bare metallic wires, conveying powerful electrical currents from different sources, be brought into contact and then gently separated, a cloud of metallic vapor may be formed between them, as in the case of the carbon electrodes.

Or if any portion of a bare wire be brought into contact with one part of a metallic conductor, and a distant part of the same wire again touches this conductor, an arc of flame may be established between the wire and the conductor.

To speak more generally, if any conducting material be placed in, or form a part of the electrical circuit, and from any cause a momentary break be made between it and the rest of the circuit, an arc of flame may result.

As these arcs of flame are very hot, their occurrence should be carefully prevented in all places except between the carbon electrodes in the lamp.

Dangers of this character may be entirely avoided by carefully insulating all the wires which carry the electrical current into or out of the building or space to be lighted.

The comparatively feeble leaping power of the currents developed by dynamo-electric machines, as already referred to, does not, however, necessitate any very high degree of insulation for the wires. Whatever the character of the insulation employed, care should be exercised to insure its being preserved intact on all parts of the wire. The removal of the insulation from but a few points of the wire might cause a dangerous discharge at such points.

When two different paths are open to the current, it will flow through both such paths. If these paths are of different conducting powers, more current will flow through the path which is the better conductor. If one of the paths be twice as good a conductor as the other, twice as much current will flow through it. If it be a hundred times a better conductor, a hundred times more current will flow through it;

divided between the different paths in proportion to their conducting powers.

Suppose, now, that two different portions of an electrical circuit be bridged over by any conducting material. Say, for example, that a wire from a line of telegraph or telephone falls across a line of electrical conducting wires, so as to connect the wire carrying the current from the machine with a part of the wire carrying the current back to the machine. If such electrical conducting wire be uninsulated, a series of cross contacts would thus be formed, and two different paths be opened to the current. Since the conducting power of the short wire, forming the cross circuit, would probably be far greater than that of the portion of the circuit with its included lamp, the greater part of the current thus "cut out" or "short circuited" would pass through the cross wire, either fusing it, or drawing dangerous arcs of flame when the contacts were at times but partial.

This danger may be entirely obviated by insulating the wire. It is therefore preferable to insulate all parts of the wires that lead either from, or to the machines producing the current.

If the human body be accidentally placed in the circuit, the partial or entire passage of the current through it may, if the machine be powerful, cause instant death, or, if the machine be of smaller size, produce a painful shock.

But to enable the current so to pass, the body must be placed in the circuit of the machine, that is, the current must be able to enter the body at one point, and to pass out and back to the circuit from another point, or, in other words, a break must be made in the wire, and the body inserted in this break.

Here again the difference between the current from dynamo-electric machines and from the common frictional machine, is to be carefully noted. Merely touching any part of a charged conductor of a frictional electrical machine, will completely discharge it, the person conducting the discharge through his body to the ground. A person touching any part of a bare conducting wire, while it is conveying a powerful electrical current, such as used in systems of lighting, would not convey the discharge through his body to the ground, because he would not be placed in the circuit. He has simply provided a place for the current to enter, but none for it to flow through him and back to the machine whence it originated.

Should, however, one portion of the wire touch the ground in any

place, especially where it is wet, then his feet might be connected through the damp ground with another part of the circuit, and two paths thus be opened to the current, with the chances of a dangerous portion thereof passing through his body.

To guard against dangers of this character it is necessary to thoroughly insulate the wire in all places where it is liable to be touched. It is far preferable to establish a continuous metallic line of conductors both to and from the machine, rather than ground the wires, and so necessitate the current to pass through the earth in order to flow back to the machine.

During the burning of lamps of the arc type fragments of highly heated carbon sometimes become splintered from the electrodes and fall below. To avoid danger from this source a cup should be placed below the electrodes to receive any fragments that may fall.

It would follow, from the principles thus briefly enunciated, that there may arise two kinds of dangers from the employment of powerful electrical currents for the purposes of artificial illumination, viz.: Dangers to property from fire; and dangers to life. With the exception of that due to falling fragments all these dangers may be readily and completely avoided by properly and thoroughly insulating the wires that carry the current to and from the machine, while that due to falling fragments may be easily avoided by the means above pointed out.

In order, however, to particularize these different kinds of dangers, and to point out more especially the proper remedy for the avoidance thereof, the committee recommend that the following precautions be taken in all cases where electric lighting is employed, viz.:

1st. That the conducting wires leading into and out of the building be suitably insulated throughout their entire extent, both to and from the machine producing the current.

2d. That an inspection be made at suitable intervals to determine whether or not the insulation has been preserved intact.

The insulation may become impaired by the following causes, viz.:

By the wires being cut by the staples or hooks used in securing the conducting wires in position.

By the wires being placed in positions subject to abrasion, either by chafing with another wire, or from any other cause.

By the wires turning sharp bends, or by being sharply bent by any cause whatever.

3d. That conductors formed of numerous short pieces of wire be avoided as far as possible, and that where their use is necessary the joined ends be made as secure as possible by wrapping, so as to prevent short arcs being formed at imperfect junctions, should the joined ends be partially separated from each other.

4th. That the wires be not grounded, that is, that no attempt be made to cause the current to pass back to the machine through the earth, but that a continuous line of wire be provided, through which the current shall so return.

In order that this precaution may be effective, the wires should not be carried near metallic bodies like lines of shafting, nor gas nor water pipes, because an accidental contact of the conductor with any of these would effectually ground that part of the wire. Where it is necessary that the wire cross such metallic bodies, it is advisable that the insulation be made better than usual at such juncti ons.

5th. That the ready occurrence of cross contacts or short circuits be avoided, as follows, viz.:

That the conducting wires from different machines, or from different parts of the same machine, be kept as far apart as convenient, and never, except when necessary, be brought nearer together than the distance between the two binding posts on any electric lamp used in the circuit.

That therefore the wire leading from the machine into the room to be lighted should leave the room as far as convenient from the place it enters.

That the wires be securely fixed in position and be not allowed to sag or bend in wide curves, except where it is necessary to permit the raising or lowering of the lamp.

That judgment be exercised in selecting the portions of the building in which to run the wires. To secure as far as possible the absence of moisture, ceilings are to be preferred to walls or floors, the latter being highly objectionable, unless the wires are placed under the flooring. As before stated, the location selected should be removed as far as possible from metallic conductors. Select the places least liable to be rendered partially conducting by moisture from any source, in which to run the wires.

6th. That the conducting wires be of sufficient size to carry the most powerful current employed without dangerous heating.

7th. To avoid the danger to life from the accidental discharge

of the current through the body, the conducting wires should in all cases convenient be placed out of reach, either by choice of location or the use of heavy and guarded insulation.

8th. That where lamps of the arc type are used, they be covered with a globe of glass, and that the lower end of such globes be furnished with a cup or pan for retaining any heated fragments.

The committee believe that if these precautions be taken electric lighting can be thoroughly safe and reliable, and that all dangers attending its use can be entirely obviated.

R. E. Rogers, M.D., *Chairman.*
Charles M. Cresson, M.D.
David Brooks.
E. Alex. Scott.
Edwin J. Houston.
Isaac Norris, M.D., *Secretary.*

REPORT of COMMITTEE of the FRANKLIN INSTITUTE on FIRE-ESCAPES and ELEVATORS.

Your committee appointed "to examine and report upon the principles which should govern the erection of fire-escapes and lifts or elevators in new buildings and on those now erected," respectfully report that they have held four meetings, have examined a large number of models, designs and plans submitted to them by inventors, have individually examined means of escape now provided in many mills, hotels, etc., and after a full discussion of the subject have reached the conclusion, that while it is impossible to specify any particular form of escape applicable to all buildings, they can nevertheless lay down some general principles that should govern the erection of stairways and other fire-escapes.

As a general rule, applicable to all buildings, the main reliance should be placed upon stairways in daily use, for the obvious reason that the occupants of a building instinctively seek such an escape in case of fire, that they are familiar with it, and that, being in daily use, it is reasonably certain to be unobstructed and to have the approaches to it clear. For these reasons, and because a stairway is usually broader and easier to descend than any special contrivance such as a

ladder, probably ten times as many persons can escape by a stairway, in a given time, than can descend by any form of ladder or stairway used exclusively as a fire-escape.

Stairways, however, as ordinarily constructed, are liable to be cut off by smoke, or by fire itself. The best plan to make them safe in this particular is to have two or more stairways accessible from every room, separated therefrom by a broad air space but connected by bridges at each story. These stairways and bridges should be incombustible and the stairways enclosed in brick walls. This plan may easily be adopted in a group of mills and even where the stairways in existing mills do not answer all the desired conditions, the building of bridges connecting isolated buildings will add to the safety of the operatives. Where there is a single building with sufficient yard space to permit the construction of stairways in isolated brick towers, approached by bridges from each floor of the mill, the same *system* of fire-escapes can be applied.

In single buildings in the closely built parts of the city, where the bridge system cannot be applied, the first and most reliable means of escape is an internal stairway or, preferably, two stairways, one at each end of the building. They should be entirely enclosed in brick walls (fire walls) and be built of incombustible materials. Their safety will be increased if they start from the ground floor and are entirely shut off by stone or brickwork from the cellar. The danger of smoke entering them in such volume as to make them useless will be lessened if the necessary doorways by which communication is had with the rooms on each floor be made low and closed by iron-encased wooden doors, closing automatically, but always free to be opened by pressure from within. Circular stairways should be avoided, and straight stairways, with turns at each floor, should also have half landings to make rapid descent less dangerous.

In a building occupying the whole of the available lot, but which is large enough to admit of the construction of two stairways, one at each end, almost absolute safety with internal stairways can be secured by the following plan: Build both stairways of incombustible materials, substantially as heretofore described, but have no openings whatever to one of them from the interior of the structure. The stairway leading into the rooms would be used for the ordinary purposes of business, both for ingress and egress. The other stairway should have openings to *external* balconies on each floor, extending

over one, two or more windows of the main building. In the event of fire, both stairways could be used, if smoke did not obstruct the one opening into the mill; but if that were cut off from any cause, all the occupants, by passing out on the balconies, could enter the other brick-enclosed stairway which, having no direct connection with the building, would not be liable to be cut off by either flames or smoke.

For the class of buildings which, by reason of their small size, can have only one internal stairway, some form of external escape is needed, for it should be laid down as a rule that one stairway or one means of escape, however good in itself, cannot, in the nature of things, insure safety. It must always be liable to be cut off by fire or smoke. Every additional means of escape gives an increase of safety; but two, if widely separated, and of themselves good, may reasonably be deemed sufficient in buildings not large enough to permit the erection of more.

Of the many devices for external means of escape none that have been submitted to your committee can be recommended for all purposes, though several have merit and may be absolutely necessary in some buildings. Your committee believe that an external means of escape should be of the nature of a stairway, not of a chute, nor an elevator of any kind, nor a car to be lowered and hoisted to high windows, nor any other kind of apparatus dependent upon moving parts which are liable to be out of order when wanted, especially when used only on rare occasions. The objection to the chute form of apparatus is that it is an unusual means of egress in which terrified people could place no confidence.

The stairway forms of escape include ladders of all kinds, but the straight ladder exposed to the weather is the most objectionable form, because women and children can make little use of it, and in winter time it might be dangerous and practically useless even to experienced climbers.

A modified form of the straight ladder, which is set away from the wall so that the latter gives support to those who are descending and which also has side guards to prevent one from falling, is an improvement on the ordinary straight ladder set against the wall. The stairway-ladder, running obliquely down a building and furnished with a hand-rail, is also superior to the straight ladder, but, to make it at all safe, some light will have to be sacrificed in most mill buildings, for it should not cross open windows and thus be liable

to be cut off by flames or smoke from a lower story. It seems desirable that, in any form of ladder or stairway exposed to the weather and built of iron, the treads should be flat, covered with a light strip of wood and perforated.

One other form of external ladder escape submitted to us had the ladder itself, together with its balconies, suspended from a rail under the cornice, so as to permit it to traverse the building and be fixed opposite any set of windows which might happen to offer the best chance of escape. While this device is open to the general objection of being movable, and therefore liable to be out of order when wanted, this objection is not of much weight since, in its normal position without being moved, it is ready for service and may be regarded as a fixture. It is as good as a fixed ladder of similar form if not moved, and it *might* be better than a fixed ladder if its position could be shifted.

Your committee does not feel called upon to decide as to the relative merits of these ladders to be used as external means of escape, further than to point out what are regarded as their general merits and defects, for the reason that they regard the manner of constructing internal stairways in daily use as of paramount importance, and look upon all extraordinary means of escape as of comparatively little value. They, nevertheless, recognize the fact that external means of escape may be absolutely necessary for some buildings, and therefore set down as principles to govern their erection that they should be of stairway form (including in this category ladders), that they should be carried up to the roof of the building to permit escape in that direction, that they should be erected on the piers of buildings and in no case cross open windows, or windows not permanently closed by shutters, and that the treads of the ladders should be flat and covered with some material, such as wood, not as likely as iron to be covered with ice or made slippery by snow in winter time.

It has been suggested that adjoining buildings in the closely built parts of the city might be provided with external means of escape by the erection of balconies connecting the windows of different buildings on each floor. If a fire occurred in one building, its occupants could go by the balconies into the other and escape by its stairway, shut off from the fire by the dividing wall. It would be necessary, of course, for the owners and occupants of such adjoining structures to consent to having this connection made, which circumstance would limit its applicability. The chief objection to it, the

danger of robbery being committed by way of the balconies, is not so serious as might at first appear, because the two buildings are generally occupied during the same hours, the balconies are exposed to view, and at night, when one or both buildings have been vacated, the windows leading to the balconies could be secured against the entrance of thieves except by noisy violence.

Your committee is aware of the existence in the city of a large number of manufacturing establishments, on the upper floors of what were formerly dwelling houses, which are exceedingly dangerous to human life. They are filled with inflammable material, are not carefully looked after, and the stairways leading to the upper floors are either unenclosed, each stairway terminating at and opening directly into the room to which it leads, or the stairways and entries are, to save space, divided from the rooms by an inch board partition. In such buildings there is special danger of fire, and if it occurs on the lower floors the stairway may in a few minutes be cut off by flames or smoke. A reasonable degree of safety can only be secured in such buildings by requiring the erection of a brick enclosed stairway, and most of them should also be provided with external means of escape.

Where such a building has no back outlet, and is so narrow that the stairway cannot be erected in the front without destroying the value of the property, a brick enclosed stairway erected in the centre of the length of the building, against the side wall and leading from the upper floors to a passage way in the cellar, but completely separated therefrom by a brick wall, would offer a means of escape to the street without sacrificing the more valuable parts of the building. Whatever the expense and inconvenience may be, however, these manufactories constructed from dwelling houses should be made safe. They are at present more dangerous than large mills and public halls erected for the purposes for which they are used, although these are occupied by a larger number of persons.

One objection to the use of external, or extraordinary means of escape of any kind, that they are liable to become obstructed, and that occupants of the building may not be aware of their existence or the manner of reaching them, can be obviated by closing up the ordinary stairway one afternoon of each week and requiring the operatives to leave the building by means of the escape. This could be done systematically on a certain day of the week without special inconvenience to the

occupants of the building, even in stormy weather, *if the escape were of practical value*, and if it were not, the sooner that fact became known the better.

The resolution under which we were appointed refers to us the question of lifts or elevators as well as fire escapes. They undoubtedly add to the risk of a spread of flames and smoke through a building, but they are absolutely necessary in large hotels and manufactories. To make them as little dangerous as possible they should be enclosed in incombustible walls, should be free from wood work about the openings in the floors, and, preferably, should be automatically closed by iron encased wooden doors when not in use.

In preparing the general report as above submitted no special attention was given to the law of the State and its requirements, the purpose being to indicate the principles which should govern the erection of fire escapes, supposing the owners of property to be untrammeled by law. It must not be forgotten, however, that the law of the State requires the erection on certain buildings (generally speaking, all buildings three or more stories in height, except private dwellings) of "a *permanent*, *safe*, *external* means of escape therefrom in case of fire."

Your committee regards these qualifying words as unwisely specific. The means of escape should be such as to satisfy an official or Board of officials charged with the duty of seeing that such escapes are erected. The chief objection is to the word *external*, for in the majority of cases internal stairways, properly constructed, afford the best means of escape, and, by some forms of construction, make external means entirely unnecessary. The word "permanent" (in the sense of fixed) may rule out otherwise useful means of escape. The word "safe" may have any meaning, according to the judgment of the individual passing upon the merits of a fire escape.

Your committee would recommend, therefore, that at the earliest possible moment the law should be amended in this particular. In the meantime property owners, however well provided they may be with escapes, will have to run the risk of the penalties imposed by the act if they fail to comply with its terms.

Several ordinances having been offered in Councils to provide for an enforcement of the law, your committee deem it their duty to suggest that provision should be made for vesting authority to pass upon fire escapes in a single department of the city government, and that it should be required to approve the plans for fire

escapes *before* they are erected, and to prescribe a form where the property owner fails to submit one. This Board should also be given large discretionary power as to the kind of escape required on different buildings, and should be required in all cases to consider the character of the building, the nature of the business carried on therein, and the number of persons liable to be exposed to the dangers of fire. A plan of escape having been approved by the proper authorities, and the escapes erected in accordance with the plans, the owner of the building should be given a certificate, to be accepted as a sufficient defence in suits for damages, provided the building and fire escapes had been maintained as designed at the time the certificate was granted. Owners of properties making alterations should be required to take out new certificates. The officers charged with the duty of examining fire escapes and granting certificates should be paid by salaries; and the fees collected from property owners, if any, should be no more than sufficient to pay the reasonable expenses, and should be paid into the City Treasury.

It is also desirable that the work of examination should either be given exclusively into the hands of the Inspectors already charged with the duty of approving plans for new buildings and alterations, or be given to those Inspectors, subject to approval, in the matter of fire escapes, by the Fire Marshal. In either case, the way to get plans examined and certificates granted should be made as simple and free from delay as possible.

In submitting this report your committee desire to add that our aim has been only to lay down such *general* principles as seemed to us wise and proper, both in regard to fire escapes and to the character of the legislation which should govern their erection. The details of the escapes can only be properly considered by reference to particular buildings, and the details of the law should be left to the consideration of our law makers.

JOHN BAIRD, *Chairman.*
R. K. BETTS.
FREDERICK GRAFF.
C. H. BANES.
STRICKLAND KNEASS.
WILLIAM D. MARKS.
HENRY G. MORRIS.
J. B. LIPPINCOTT.
ISAAC NORRIS, M.D., *Secretary.*

CHEMICAL METHODS FOR ANALYZING RAIL-STEEL.

By MAGNUS TROILIUS, Chemist to C. P. Sandberg, London, Eng.

Read before the American Institute of Mining Engineers, October, 1881.

Continued from page 358.

By comparing these formulas with those of the bromine and ammonia process, I think a pretty clear view may be had of the different conditions of the two methods, without further comment.

After what has just been shown, there is no disguising the fact that the bromine and ammonia process is more rapid and convenient than the process with fixed alkaline salts; but it should be here pointed out that a great mistake is often committed in saying that it is impossible to free the precipitate from alkali by washing. Professor Eggertz has stated that by means of cold water containing one per cent. of hydrochloric acid the precipitate may be easily freed from alkali. In Germany, where the fixed alkali method is used, this mode of washing is not so frequently known as might be supposed, and at one works I was told that experience went to show that by using chlorine instead of bromine the washing of the precipitate with ordinary hot water could be more easily effected.

When chlorine is used for precipitating the manganese, no ammoniacal salts must be present, otherwise the explosive compound Cl_3N may be formed. On the other hand, in the case of bromine and ammonia, no explosion need be feared.

The different methods, of which the outlines have now been given, yield quite concordant results when carried out by experienced hands, and for rail-steel there is certainly no preference to be given to any of them so far as accuracy goes. According to my own working, the results should not vary more than 0·05 per cent. and the time required for an estimation one day; of course several assays can be made at the same time. As to rapidity, the bromine and ammonia process will no doubt prove the best, for reasons stated above.

There are many volumetric methods for determining manganese, but, as far as I have seen, none is more rapid than the gravimetric method with bromine and ammonia. A new volumetric method has been recently devised in the Stockholm School of Mines and will shortly be published.

Sulphur Determination.

I dissolve 5 grammes of steel in aqua regia and separate the silica in the usual way. In the boiling solution, the sulphur is precipitated by means of 2 cc. of a concentrated solution of chloride of barium. Boiling is continued for a short time and the solution is then left to stand during one night.

The sulphate of baryta, before being taken upon the filter, is decanted repeatedly with hot water. Some drops of hydrochloric acid must be added to prevent oxide of iron from being precipitated. By washing carefully in this way accurate results are secured, always provided that the reagents are pure. The purity of the reagents is, indeed, the difficulty in this method, as it is almost exceptional to find the acids bought as "special" free from sulphur. The sulphur must be estimated in the reagents and the necessary deductions made.

There are chemists, however, particularly in Germany, who assert that even with pure reagents you will get too high results by the aqua-regia method. They therefore use the bromine method, leading the gases from the steel dissolving in dilute hydrochloric acid through a solution of bromine in hydrochloric acid. The sulphuretted hydrogen is thus oxidized and can be precipitated in the usual way, by means of chloride of barium.

As far, however, as I have been able to see, the bromine method gives too low results. I have had to determine the sulphur in steel with the aqua-regia method against chemists using the bromine method, and on some occasions I have found 0·08 per cent., while the others have found only half of this, 0·04 per cent. By means of the Eggertz silver plate, however, I easily ascertained that 0·04 per cent. was much too low. The experiment was carried out by hanging a clean, small silver plate over the gases evolved from 0·1 gramme of steel, dissolving in 1·3 cc. of sulphuric acid of 1·23 sp. gr. The plate then got a decidedly more bluish than brownish color, whereas if the percentage of sulphur had been only 0·04 per cent., the color would have been simply brown and not blue at all. This plate method was worked out by Professor Eggertz, and has been for many years in use at all blast furnaces in Sweden for the daily testing of every cast of pig iron. The plate method will yield very good results for percentages of sulphur between 0 and 0·4 per cent., and this is quite sufficient for Swedish irons. But for sulphur over 0·04 per cent., the method is less accurate, except in extremely well-trained hands,

and one can only make a rough estimation, as a rule. The relations of the colors of the plate to the percentage of sulphur is shown by the following table:

No color, . . .	0·00 per cent.
Slightly yellowish, . .	0·01 "
Yellow, . . .	0·02 "
Yellow-brown, . .	0·03 "
Brown, . . .	0·04 "
Blue, . . .	0·20 "

Between brown and blue there are numerous variations in color. A certain red tint signifies 0·10 per cent., but this can only be learnt by practice.

APPENDIX.

Color Test for Carbon in Iron and Steel.—By V. Eggertz.

(Translated from the Swedish by M. Troilius.)

The first description of this method appeared in the *Jernkontorets Annaler*, of 1862, page 54, and in the *Berg- und Hüttenmännische Zeitung*, 1863, page 373. Subsequently, several additions were made to this description in the *Jernkontorets Annaler*, 1874, page 176, and in the *Berg- und Hüttenmännische Zeitung*, 1875, page 140. At that time I considered that it would be practically sufficient to determine the carbon in tenths of a per cent., but I now find that even hundredths of a per cent. are required.

For commercial purposes, however, iron and steel is generally stamped with only whole or half tenths of a per cent. of carbon, it being chiefly for the softer irons and steels with carbon between 0·10 and 0·25 per cent., that the greater accuracy in determing the carbon is required.

Pure hydrate of oxide of iron, containing 0·1 gramme of iron and free from chlorine, will give a yellow-greenish solution when dissolved in 2·5 cc. of nitric acid, specific gravity 1·2. The solution will lighten somewhat on addition of 1·5 cc. of nitric acid, but not so much as when water is added instead of nitric acid. When hot, the solution has a much darker color than when cold. By adding 4 cc. of water in either of these cases, so as to make the bulk up to 8 cc., the iron color will be totally got rid of. Hence the rule for color tests for

carbon, that the solution of iron must be diluted with a volume of water at least equal to the volume of nitric acid used for dissolving the iron, and that the volume of the solution must not be smaller than 8 cc.* when the color is to be observed.

The nitric acid as well as the water must be entirely free from chlorine or hydrochloric acid, which otherwise will give a yellowish color† to the solution, even if present in only very small quantities. The quantity of nitric acid should correspond to a certain extent to the amount of carbon supposed to be present in the iron. For iron containing carbon 0·25 per cent., 2·5 cc. are thus used for each 0·1 gramme, for 0·3 per cent. 3 cc., for 0·5 per cent. 3·5 cc., and, finally, for 0·8 per cent. 4 cc. For steels with more carbon 5 cc. should be used. The last-named quantity is also to be used for white pig irons, of which, however, 0·05 gramme only are carefully weighed out for each test.

When the amount of carbon is quite unknown 2·5 cc. of nitric acid are added to start with, and then more until it is indicated by either the color or by the quantity of separated carbon-substance that no more acid is wanted. A little more acid than the quantity just mentioned does no harm, if only an equal volume of water is afterwards added. Thus for white pig iron 7 cc. may well be taken so as to prevent a too quick precipitation of organic matters after diluting. With too little acid a too dark solution will be obtained. Thus, if 0·1 gramme of steel containing 0·8 per cent. of carbon is dissolved in 2·5 cc. of nitric acid only, instead of 4 cc., the color of the solution will, after dilution, represent a percentage of carbon equal to about 0·9 per cent.

The iron for testing is to be finely divided either by filing,‡ by aid of a good clean file, or by means of a boring machine (which is preferable), by rubbing with some hard steel edges, or by crushing in a steel mortar, if it is very hard.

The test-tubes for dissolving the steel should have an internal diameter of 15 mm. and a length of 120 mm. The tubes are to be cleaned

* For each 0·1 gramme of iron.—M. T.

† Only 0·0001 gramme of chlorine produces a distinct yellow color in a solution of 0·1 gramme of iron (from hydrated oxide of iron) in 2·5 cc. of nitric acid. This color can be noticed even on addition of 1·5 cc. of nitric acid and 4 cc. of water. Upon further diluting, the coloration becomes less distinguishable.

‡ Worn-off particles from the file may, however, greatly increase the percentage of carbon in the iron.

out by means of coarse filter-paper rolled together and well cut at the edges. 0·1 gramme of the iron or steel for testing, or in the case of white pig, 0·05 gramme, carefully weighed, is then put into the tube and 2·5 cc. or more of nitric acid of specific gravity 1·2 added. The acid may be readily measured off in a little measuring glass of 10 mm. diameter and 75 mm. length, and graduated so as to show whole or half cubic centimetres. The tubes are covered with small watch-glasses (23 mm.) and put into a cylindrical vessel of copper-plate having a height of 100 mm. and a diameter at least of 120 mm. This vessel is covered with a copper plate provided with a thermometer and holes for the tubes. The holes are marked with engraved figures. The vessel is to contain water and a few grammes of paraffin to prevent the evaporation of the former, and is heated to 80°C. by means of a gas flame. This temperature should be kept up all the time. The tubes are shaken now and then, and the solution is completed when no further gas bubbles appear. One and a half to two hours, or sometimes longer, time is required to effect complete solution, the time depending upon the amount of carbon in the iron.

Up to the present time this has been the ordinary way of proceeding, but it is shown by experience that the keeping up of the temperature of 80°C. requires more care than has been generally taken. Of course, this has been of less influence as long as fresh weighings of the standard steel have been dissolved along with every set of samples for testing. But if it is desired to avoid the frequent dissolving of standard steel and to use permanent standard solutions (see further down) then the iron must always be dissolved at the same temperature. For this purpose it is the safest way to put the tubes into boiling water. By this means the time for dissolving may be shortened to about three-quarters of an hour, and the color of the fluid will then turn out a little darker than by dissolving at 80°C. If, occasionally, greater rapidity is required the solution may be effected by boiling over a lamp flame; between the flame and the tubes there should be put a brass gauze with the tubes resting on it. The color will in this case generally be somewhat darker than when dissolved at 100°C. The reason for heating to 80°C. instead of 100°C. has been, that in the latter case there is sometimes a reddish-yellowish deposit formed on the glass; when brought down into the fluid this matter has made the solution turbid. The deposit must

be dissolved by shaking if possible, otherwise it must be filtered off. It consists of nitric acid and oxide of iron. This is borne out by the fact that it also arises by heating a solution of pure hydrate of oxide of iron in nitric acid.

All gas bubbling having ceased, the tubes are taken out of the hot water and put into a beaker containing cold water. They should be covered in some way so as to be totally excluded from the daylight, which would soon give too light a color to the solutions. If protected in this way the solutions will retain their color for many days. The burette for determining the carbon should have 30 cc. capacity and be graduated into tenths of cubic centimetres and be provided with one large and one small mouthpiece at the upper end. The solution is brought into this burette through a filter, if necessary, as for instance if the fluid is turbid or graphite is present. Then distilled water is to be added; the quantity of water, including the water used for cleansing the test-tube, must be at least equal to the quantity of nitric acid used, and the total volume must not be less than 8 cc. when it is to be compared with the standard solution. The standard solution is prepared by dissolving standard steel. It should be diluted with water so as to make each cubic centimetre of the same to correspond to 0·1 per cent. of carbon. It may also be carefully diluted with more water so as to have each cubic centimetre =0·05, 0·02, 0·01 or 0·005 per cent. of carbon, of 0·1 gramme of iron or steel used. For example, dissolve 0·1 gramme of standard steel, containing 0·8 per cent. of carbon, in 4 cc. of nitric acid and add water to 8 cc. The solution is to be carefully mixed after each addition of water; otherwise the lower part of the fluid will appear too dark. After mixing, at least one minute should be allowed for the fluid to run down along the sides of the burette before reading off, which is done at the upper border of the fluid.

The above-mentioned standard solutions, representing the total, half, fifth, tenth and twentieth of the standard, may be marked and used as follows:

	Percentage of carbon per cc. for 0·1 gramme of iron.	To be used for iron with a percentage of carbon =
N	0·10	0·8 and more
$\frac{1}{2}$N	0·05	0·4 —0·80
$\frac{1}{5}$N	0·02	0·16—0·50
$\frac{1}{10}$N	0·01	0·08—0·25
$\frac{1}{20}$N	0·005	0·04—0·08

The smallest percentage of carbon* found in iron in this country is 0·04 per cent. A good daylight is required when using the last-named standard solution, and this remark applies to all colorimetric tests.

The dilution cannot be carried farther than twenty times the original solution in the case of using the ordinary burettes of about 12 mm. diameter. But one can sharply distinguish the difference in color between ordinary distilled water and a standard solution diluted to forty times its original volume and, therefore, if one uses burettes of 24 mm. diameter, it will be possible to approximately determine the carbon in iron containing only 0·02 per cent., supposing such soft iron to be produced. In this case measure off 1 cc. of the "N" solution in a delicate measuring tube and dilute to 40 cc. Thus 1 cc. will be equal to 0·0025 per cent. of carbon. Pour this standard solution into the 24 mm. tube. This tube should exactly correspond with another graduated 24 mm. tube, in which 0·4 gramme of the iron for testing is to be dissolved in 10 cc. of nitric acid, the solution diluted to 32 cc. and then more until the colors agree. For instance, if the colors agree at 35 cc. bulk, you find the percentage of carbon thus:

$$0{\cdot}0025 \times 35 \div 4 = 0{\cdot}022 \text{ per cent.}$$

Hitherto it has been the usual practice to compare colors by holding a piece of thin filter-paper behind the tubes. In this way, however, one is much dependent on the distribution of light in the room, which, therefore, ought to have only one window for this purpose. For most people's eyes the left-hand tube will appear somewhat darker than does the right-hand tube. For this reason it was originally prescribed that the tube containing the solution of iron or steel for testing should be kept to the right so as always to have it in the same position. In order to avoid being dependent on the room I work in, and to lessen the difference in color between the left- and right-hand tubes, I now most advantageously use a contrivance in the shape of a little camera, into which the tubes are put. The camera is made of wood 6 mm. thick, and is open at both ends. The inner sides are blackened. The internal height is 80 mm., width of forepart 26 mm. and width behind 120 mm. The tubes are put in the forepart through apertures in the upper part of the camera and steadied at the bottom by a gutter of copper plate and at the upper part by a brass wire. The

* By means of the iodine method 0·038 per cent. of carbon was once found in 8 grammes of a Swedish Lancashire iron.

box is closed at the wider end by means of thin filter-paper* nailed to it. Burettes and tubes should be closely of the same quality as to color, etc., and be wiped with clean linen before being put into the camera.

We have of late been using as standard steels two Bessemer steels, one of which contains ·80 per cent. and the other one ·16 per cent. of carbon. These steels are kept in pieces 12 mm. square. Samples from these pieces are always taken at right angles to the longitudinal axis. The carbon in these steels was determined by me by aid of the iodine method, as described in the *Jernkontorets Annaler*, 1862, page 47. 5 grammes were taken for each test. Three analyses of the harder steel showed ·79, ·80 and ·82 per cent. of carbon, and two analyses of the steel containing ·16 per cent. agreed very closely. These carbon determinations also agree very well with those of Dr. A. Tamm, whose results by combustion are described in the *Jernkontorets Annaler*, 1874. The iodine method is now modified, so that instead of using iodine direct for dissolving the iron or steel, a solution of iodine in iodide of iron is used. This is on account of the difficulty of obtaining pure iodine. The solution now used is prepared by dissolving 10 grammes of iron in 50 grammes of iodine and adding another 50 grammes of iodine, which is readily dissolved. The solution is filtered and water passed through the filter until a bulk of 100 cc. is reached. 10 cc. of this solution are required for 1 gramme of iron. With regard to the diminution in weight of filter-paper on being treated with acid, we have generally used platinum filters for collecting the carbonaceous matter. However it is somewhat difficult to obtain good platinum filters and, therefore, it seems preferable to use filter-paper which has been treated with hydrochloric and hydrofluoric acids,† whereby the inorganic ingredients are almost completely extracted. A filter of 60 mm. diameter thus treated does not give more than 0·0001 gramme ash. The carbonaceous mass should be dried on a water-bath at a temperature of 95°—98°C. For this purpose the mass is put into a crucible, and the crucible then put into a glass tube, closed at one end and having a length of 130 mm. and diameter of 35 mm. The upper end of the tube is closed by means

* This can also be effected by applying a screen holding a glass adapted to the eye as to convexity or concavity, and having about 40 mm. diameter.

† Fresenius' Zeitschrift, 1879, p. 582.

of a cork, through which hangs a thermometer. The crucible may be inserted or removed by means of a crooked brass wire.

Even for white pig iron better results than could be expected* are now obtained by the color test. The difficulty in this case lies in the use of only 0·05 gramme, and still diluting to a very large volume, which renders a slight error in observation very important. The solutions of white pig iron should be rapidly read off, for the reason that they soon become turbid with a precipitate of organic matter. This inconvenience may, to a great extent, be removed by using 7 cc. instead of 5 cc. of nitric acid.

The mode in which the carbon is present in the steel as "cement carbon," or "hardening carbon," does not influence the color test otherwise than to render the solution less dark in the latter case, when the carbon is more intimately combined with the iron. Thus a steel containing 0·8 per cent. of carbon showed only 0·55 per cent. after strong hardening and crushing. After reheating the steel to a low red heat, the original percentage of carbon was again obtained. By aid of the iodine method, 0·8 per cent. of carbon was found in 5 grammes of the hardened steel. A piece of iron containing 0·3 per cent. of carbon, upon being hammered down cold from 12 to 6 mm. square, showed the same carbon before and after hammering.

Different kinds of iron and steel behave very differently during dissolving, some solutions at once becoming colored, others only after heating. The final color, however, after diluting with water, generally tallies very well with the color of the standard. In case there should be any difference in tint—yellow or brown—it is best to notice the intensity of colors, which can be done by holding a thin filter-paper behind the tubes and observing through which tube one can most easily see certain dark points in the paper, etc. It is a matter of course that some persons are able to use the color test with greater advantage than others, according to their different eyesights; but eyesight may be considerably improved in this respect by practice, as shown by experience; in fact, only very few have hitherto been found incapable for this work.

The following experiments have been made with regard to the influence on color exercised by foreign substances in iron and steel:

Manganese.—0·05 gramme, contained in carbonate of manganese,

*Jernkontorets Annaler, 1874, and Berg- und Huttenmannische Zeitung 1875, p. 440.

was dissolved in 2·5 and 5 cc. of nitric acid. The color thus obtained was brown, owing to the presence of a little oxide of manganese. After heating to 100°C. a small precipitate was obtained (in all probability hydrated dioxide), and the solution assumed a slightly reddish color, which disappeared after diluting with water to 8 or 10 cc. These experiments show that the color test may be used for determining the carbon in ferromanganese. A ferromanganese containing 80 per cent. of manganese showed 4 per cent. of carbon by color test.

Phosphorus.—0·001 gramme, contained in phosphate of sodium, was added to a solution of 0·1 gramme of iron (as hydrated oxide) in 2·5 cc. of nitric acid and 2·5 cc. of water; no difference in color was noticed after this addition. Iron containing 5 per cent. of phosphorus is difficult to dissolve, and with 10 per cent. of phosphorus it is insoluble in nitric acid.

Sulphur.—0·001 gramme, contained in sulphate of magnesia, was added to a solution of iron in 2·5 cc. of nitric acid and 2·5 cc. of water. The color of the solution remained unaltered.

Neither did any alteration in color take place when 0·01 gramme of manganese, 0·01 gramme of phosphorus, and 0·001 gramme of sulphur (all contained in the above-mentioned salts) were added to a solution of 0·1 gramme of iron (as hydrated oxide) in 5 cc. of nitric acid and 5 cc. of water. The same quantities, when added to a solution of 0·1 gramme of standard steel with 0·8 per cent. of carbon, did not alter the color here either.

Copper.—0·001 gramme was dissolved in 2·5 cc. of nitric acid and 2·5 cc. of water. No coloring of the solution.

Silicon in iron and steel is, to a very great extent, dissolved in nitric acid on heating, even if lumps of silica be seen floating about at the beginning of the dissolving process. 0·4 per cent. of silicon in steel has no influence whatever on the color test. Silicious pig irons always contain graphite, which must be filtered off, together with the silica which may possibly have remained insoluble.

Tungsten in iron or steel is converted into tungstic acid (WO_3) when the iron or steel dissolves. It is insoluble, and must be filtered off.

Chromium.—0·002 gramme, contained in hydrated oxide, when dissolved in 2·5 cc. of nitric acid and 2·5 cc. of water, gave a grayish-bluish color to the solution. After addition of water to the double

volume this color grew less distinct. On addition of more water the color gradually grew fainter, and at 40 cc. it disappeared. It is difficult or even impossible to dissolve in nitric acid iron or steel containing much chromium.

Vanadium.—0·001 gramme, contained in vanadic acid (V_2O_5), when dissolved in 2·5 cc. of nitric acid, gives a faint yellow color to the solution. This color disappears after adding 2·5 cc. of water.

Nickel.—0·001 gramme, when dissolved in 2·5 cc. of nitric acid, gives a green color to the solution. This color remains visible after adding 2·5 cc. of water, but disappears at 8 or 10 cc. volume.

Cobalt.—0·001 gramme, when dissolved in 2·5 cc. of nitric acid, gives, as is well known, a red color to the solution. After adding water to 24 cc. this color is hardly visible, but may, however, not be considered removed before diluting to 40 cc.

In my paper of 1862 I pointed out the desirableness of having permanent standard solutions of *inorganic* matters, instead of those prepared by dissolving burnt sugar in spirits of wine, the latter solutions getting lighter after some time, particularly after prolonged exposure to the sunlight. Many propositions have been made of late, in various journals, with regard to this matter, and compounds of iron, cobalt and nickel, as well as potassium bichromate, have been recommended for the purpose. I have experimented upon several such mixtures, and lately, at the suggestion of Professor F. L. Ekman, I have tried the chlorides of iron, cobalt and copper, and I find that these yield the best results, as they allow of the production of any tint in yellow, brown or green. Such mixtures, diluted with water containing 0·5 per cent. of hydrochloric acid of 1·12 specific gravity, have been found to remain unaltered even after a long exposure to the sunlight. The mixture grows more and more yellow by adding hydrochloric acid drop by drop only. I prepare my standard solutions as follows: By adding to the neutral chlorides water containing 1·5 per cent. of hydrochloric acid for the chloride of iron, and 0·5 per cent. for the two other chlorides, I prepare solutions of a strength corresponding to 0·01 gramme of metal per cubic centimetre. Then 8 cc. of the nitric solution are mixed with 6 cc. of the cobalt solution and 3 cc. of the copper solution, and about 5 cc. of water containing 0·5 per cent. hydrochloric acid are added to the mixture. At a temperature of 18° C.* this solution shows exactly the same color as a

* This is an addition made since publishing the paper in the *[illegible]*
—M. T.

solution of steel in dilute nitric acid corresponding to 0·1 per cent. of carbon per cubic centimetre. The solution may afterwards be diluted with water, containing 0·5 per cent. of hydrochloric acid, to any standard color required. The addition of water is almost directly proportional to the percentage of carbon. It need scarcely be mentioned that the quantities, 0·1 gramme for iron and steel, and 0·05 gramme for white pig iron, must be correctly weighed out when artificial standard solutions are used.

Now that permanent standard solutions can be obtained, it seems to be the right time to apply the old method for the determination of copper in ammoniacal solutions. This method was adopted by Mr. J. Blodget Britton, of Philadelphia (*Fresenius' Zeitschrift*, 1871, p. 245),† for colorimetric tests on iron and steel. He uses several tubes of the same size and quality, having equal volumes of standard solutions of different strengths in the different tubes, and dissolves the steels for testing in similar tubes, and finally dilutes these solutions with water to the same volume as the standard solutions. By comparing these he is able at once to determine to the hundredth of one per cent. by using 1 to 2 grammes for each test. He used burnt coffee for his standard solutions, which he considered to be better than burnt sugar.

Greater accuracy in color tests may also be obtained by looking at the solutions in the tubes from above, instead of from one side. In this case, however, the bottoms of the tubes must be perfectly alike. A suitable light must be provided, and the columns in the tubes must all have the same height.

I trust that a strict observation of the directions now given will essentially remove the difficulties which have been experienced in using the color test, as well in this country as abroad. It should also be borne in mind that the properties of iron and steel are not dependent on the amount of carbon only.

Before concluding this paper I have to thankfully acknowledge the service rendered by Mr. C. G. Dahlerus, mining engineer, in working out the results now published.

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* From Mr. F. O. Söderberg, at the School of Mines, Stockholm, standard solutions, artificially prepared, as well as standard steels, are obtainable. The permanent solutions are kept in close glass tubes, to each of which belongs a corresponding burette. Those having burettes may send them to him in order to get suitable tubes for the standard solutions.

NOTES ON THE PROPERTIES OF DYNAMO-ELECTRIC MACHINES.

By Elihu Thomson.

The subject of the properties of dynamo-electric machines attracts, at present, considerable interest. Perhaps, therefore, the following observations may be of interest as exhibiting some unusual phases of their behavior. The machines with which the experiments were made were of my design and consist of an armature of spherical outline revolved between two magnet poles, constituting two nearly hemispherical concavities enveloping the armature. The field coils are two in number and surround the field cores and also the armature itself. The armature has three coils only wound upon it, and the commutator, three insulated segments of a ring, the slots between the segments allowing them to overlap, so that each segment covers over 160° of the outside circle of the commutator. A full description of this machine and its performance is in preparation. The machines experimented with could sustain from four to ten arc lights in series, at about 800 revolutions per minute.

The first experiments to be described illustrate the tendency of the magnets, which were of cast iron, to prolong the generated current even after stoppage of the machine.

Motion of about 150 revolutions per minute was given to the machine while on closed circuit, until it had charged its magnets and had shown, in consequence of a development of current, a considerable resistance to rotation. It was then suddenly stopped and immediately thereafter its circuit opened, when there appeared a small but distinct spark at the break. Other evidences of the circulation of current were obtained immediately after the complete stoppage of rotation. A smart shock could be felt by grasping the wires on each side of the break in the circuit at the time it was made.

The origin of this after-current or current after stoppage of the machine is, of course, the gradual loss of magnetism of the field-magnet cores, which induce a current in the encircling wire during the time that such magnetic change is taking place. The duration of the current is about $\frac{1}{2}$ second, but increases with the size of machine

used. The current, of course, endures only until the machine magnets have lost all except what is termed their residual or permanent magnetism.

A more curious effect, however, is produced by putting the same machine, with its bearings perfectly free and well oiled, upon closed circuit and then revolving it as before at a moderate speed. The motive power is now suddenly removed, as by quickly throwing off the driving belt. The armature of the machine in this case rapidly comes to rest and then, singularly, takes about $\frac{3}{4}$ of a turn in the opposite direction.

I have repeatedly verified this result, but do not know whether it would be obtained with other dynamo-electric machines. The armature must be very light and free. Here, again, the residuary current not only stops the machine but reacts upon it so as to work it for a moment as a motor in the contrary direction—just as if a current had been applied from a source outside the machine. What, then, is the source of this current which is able to reverse the movement of the armature? It is, as before, a current generated by and continued during the loss of magnetism of the field-magnets of the machine, and this current is, of course, in the same direction as when the machine was revolved by the applied power.

Another observation made with these machines was as follows:

A single machine driven by power was connected to two others in multiple arc, so as to drive them as motors; but these latter, although relatively nearly alike in proportion, were of different sizes—the electromotive force of one being double that of the other when driven at equal speeds. The smaller could maintain four, and the larger eight arcs in series, the character of the arcs being alike. When joined up to the first machine or generator, in parallel circuit as above indicated, both the other machines acted as motors and revolved. The speeds which they acquired, while not performing work, were, however, very unequal, and were found to be approximately in the *inverse ratio of their electromotive forces as generators when driven at an equal speed.* This observation was repeated with other sizes with results very nearly agreeing with the above statement. It would seem from the above that both machines, working as motors without retardation other than air resistance and friction, attain such speeds that the counter-electromotive forces developed by them are nearly equal and the current traversing each of them also nearly equal. The different electrical

resistances of the two machines is no doubt a modifying influence, but it seems to have little effect and, in fact, counts for little in comparison with the counter-electromotive forces developed under the conditions.

The effect of running the two machines in series as motors is very different. When so used, and with their shafts very free to turn, the larger, or one capable of producing the highest counter-electromotive force to that of the generator, alone revolves, while the smaller remains at rest. If, however, the former or larger machine be made to perform mechanical work—that is, to furnish power—its running speed is, of course, retarded and the other machine turns with a speed depending on the amount of work abstracted from its companion.

New Britain, Conn., Nov. 6, 1881.

BLAST-FURNACE HEARTHS AND LININGS.

By John Birkinbine.

Read before the American Institute of Mining Engineers, May, 1881.

The high temperatures which of necessity must be maintained to smelt iron ores require that the material forming the inside of the blast-furnace, and the bottom, should be sufficiently refractory to withstand the destroying action of intense heats, and not readily susceptible to the dissolving action of the cinder resulting from the furnace operation.

In the older furnaces sand-rocks, slates and steatites were employed, the ordinary practice being to lay a bottom of sand-rock, neatly dressed and jointed, and upon this the crucible and boshes were constructed of the same material, the shaft of the furnace being laid with slate or shale. The sand-stone bottom and crucible are at present in use in a number of charcoal furnaces, and shale inwalls are still found in this country. Professor Akerman states that in some Swedish furnaces bricks moulded from furnace slag are used in lining the shaft. The stone lining of the shaft was gradually abandoned, ordinary clay brick being sometimes substituted, but firebrick are now mostly employed. Firebrick were first used for the inwalls or the shaft, then they were used for boshes, after that they were laid in the crucible, and finally bottoms were made of them, until at present their use for lining furnaces

throughout is almost universal. The firebrick are no more refractory than some of the sandstones, shales or soapstones formerly in use, but they possess several advantages.

Large bottom and crucible stones were expensive to shape, handle and to place in position; they were often cracked or spalled by sudden changes of temperature, and, owing to the joints running through them, were more apt to permit gas, cinder or metal to work out by the crevices formed by expansion of the crucible. Firebrick, on the other hand, on account of the ease with which they are formed into convenient shapes, are more readily transported, handled and placed in position; if properly made they are not apt to crack, and, by laying them so as to break joints, few continuous crevices are formed, or if formed they are generally small. Other great advantages in the use of brick are the facility with which repairs can be made without taking out an entire wall, the possibility of using thinner walls than when stone is employed, and the construction of more shapely furnaces. Where stone was employed any considerable cutting or wear necessitated the removal of the entire work, or so much of it that the furnace had to be blown out, and in such case the complete relining of the stack generally followed. But by the use of brick and blocks from refractory clay repairs of considerable magnitude have been made while the furnace was in blast, or when blown out a few matches have restored the crucible and boshes to good condition, in which a long blast has been made. The repair of an inwall by blowing down part way, and carrying off the gases and much of the heat by means of a wrought-iron tube packed around with fine ore or clay, is not unusual,* and there are on record instances where an entire crucible has been renewed while the furnace was in blast by cutting out† sections of the old work and replacing it with new until the whole was completed.

Firebrick, as now made, are the best known material for use in the interior of a blast-furnace, and yet it cannot be said that they have fully met the requirements of the work to which they are applied. This, in some instances, is undoubtedly due to the chemical composition of the brick and their physical structure; in others it is owing to the manner of operating the furnaces. But it must be admitted that in the present state of our knowledge we have nothing which is econo-

* *Vide* Translations of the American Institute of Mining Engineers, vol. iv, p. 29.

† *Vide* Translations of the American Institute of Mining Engineers, vol. v, p. 92.

mically applicable for lining blast-furnaces which withstands the fusing and dissolving action of the heat and fluxes. It not only requires a material infusible at the temperature of molten iron, but one infusible under the action of an immense blow-pipe flame, generated at the tuyeres. At a late meeting of the United States Association of Charcoal Iron Workers a discussion took place upon the question whether firebrick crucibles could be used in cold-blast charcoal furnaces, as most of these plants still employ sandstone crucibles. Now, the fusing action in a cold-blast furnace is apparently less severe than in furnaces using hot-blast. True, the crucibles are generally small, but the pressure of the blast is very slight. Evidently, then, the indifferent success which has followed the use of firebrick in connection with cold-blast charcoal practice is owing more to the dissolving of the bricks, aided by the presence of considerable quantities of potash, on account of a poverty of flux than to a want of refractory character of the material.

Looking at the subject in a purely physical light, and disregarding any chemical action, the firebrick, if capable of withstanding the high heats without softening, should last longer in the lower zones of the furnace than in the upper, for the materials charged are much reduced in size and volume, and also softer when near the tuyeres, and the wear should consequently be much less. There are instances of long-continued furnace campaigns, but the present average blast will not reach two years. The solvent action on firebrick appears to be less appreciated than it should be, and evidently much of what is termed wear by stock is due more to chemical disintegration than loss by attrition. The most rapid driving in a blast-furnace now recorded represents a descent of stock at the rate of 8 feet per hour; and this is exceptional. A movement so slow would require an extended period to materially affect, by wear, even a soft substance; but, as the rate of descent in most furnaces is but 2 or 3 feet per hour, much of the loss charged to wear by stock seems unaccounted for by materials moving against the lining at the rate of 3 or 4 miles per annum. The throat of the furnace has often been damaged by dumping large pieces of stock against it; but, even where such is not the case, the upper parts of the inwalls in furnaces have sometimes to be renewed, owing to the gases resulting from the furnace operation acting upon the brick and destroying them.

One of the most notable instances of this was where the 16-inch wall

of the tunnel-head above the stock-line was destroyed and replaced. After lasting for but seven days, the new work had similarly failed. During this time the furnace worked badly, and when blown out was found scaffolded. It is doubtful if there is much risk of damaging the lining at the tunnel-head by gases when the furnace is working normally, and the destruction of the firebrick above the stock-line seems (as far as opportunities offered for investigation have shown) to have accompanied irregular operation.

Again, there are instances where firebrick have given out in one furnace, while others, made by the same works, and even taken from the same kiln, have proved most satisfactory. As the brick were so laid as to be practically under the same conditions as to thickness of walls and intensity of blast, we can scarcely assume that the temperature was higher in one furnace than in the other, and must, until we are able to determine to the contrary, refer the destruction to chemical action. In one instance, where such difference of behavior occurred, both furnaces were under control of capable managers expert in chemical manipulation, and were closely similar in proportions, working on similar ores, flux and fuel.

Another point in favor of the theory of chemical disintegration is the rapid loss of the bottoms of furnaces. As these are protected, except at brief intervals when casting, by a liquid bath of iron and cinder, we would naturally look for their remaining intact for a long time; but the bottom is often quickly destroyed, several feet having at times been lost in a year's work. An instance is recalled where the bottom of a blast-furnace wasted away four feet in twelve weeks. Cases could be cited where several feet of the bottom have been lost in a few hours, but this was caused by physical rather than chemical action, either from defective construction or faulty manipulation. The well of the crucible, or that part below the tuyeres, is subject to the same action as the bottom, and is often cut or wasted away rapidly. Undoubtedly, the temperature maintained naturally assists and intensifies the chemical action referred to, and the firebrick are wasted or dissolved away more rapidly, owing to the high temperature prevailing.

But, admitting the affinity of the silica of the firebrick for the bases in the charge, the question naturally recurs, Are firebrick, as ordinarily made, infusible at the temperature existing in a blast-furnace crucible? The present practice is to dispense with thick walls and use in their place thin ones, even as low as 16 inches for the crucible. Now,

if fluxing action were the only or most potent damaging force at work, the thicker the walls the better, for then there would be more to disintegrate; but we find in practice that a wall must be thin and exposed to the air, or subjected to artificial cooling for protection. This naturally points to fusion, and this view is verified by the appearance of a furnace when blown out.

It is often said that the interior of a furnace glazes in the first part of the blast, thus preserving the bricks. It may be possible that, owing to the excess of fuel, a temperature exists during the early days of a blast which forms a fusible slag on the face of the brick, infusible at the ordinary working temperature of the furnace; but it is more probable that the brick are not infusible at the ordinary heat in the furnace, and that the glazing exists only when the temperature falls below this point, as when the furnace is blown out. This may be illustrated by the ice film which forms on bricks covered with moisture when the temperature is low. The minimum thickness of brickwork for crucible walls seems to be determined; first, by sufficient stability; second, by having the walls, if possible, only so thick that they will conduct away the heat produced against the inner face so fast that that face may be kept below the fusing-point, yet without too great loss of heat from the crucible. The employment of braces binders and buckstaves permit of quite thin walls being employed, and, as far as stability is concerned, quite thin walls are practicable. There may be exceptional cases where a comparatively long campaign has been made by a furnace without seriously cutting away its walls; but, upon the termination of a blast, the crucible and boshes are generally found much reduced in thickness and the remaining portion protected by a deposit of dust or graphite. Instances have been noted where but four inches of firebrick remained when the furnace was blown out, but this thin wall was well protected by an inner accumulation.

Recognizing the advantages gained by thin walls, efforts have been made to preserve them by extraneous means. They have been secured by a series of wrought- or cast-iron buckstaves, and jets or sprays of water have been thrown against the intervening brick masonry. They have been encased by a sheet-iron jacket, enclosing coils of pipe through which water circulated. They have been protected by cast-iron water jackets, either as hollow boxes, or containing coils of pipe, the jacket serving the double office of sustaining the crucible as well as cooling

the walls. Horizontal circles of cast iron, having water pipes cast in them, have been placed around the crucible and boshes at intervals in the brick-work, and coils of pipe have been built into the walls. The success which has attended these efforts demonstrates that, to hold the crucible and bosh walls, artificial cooling must be resorted to.

The manner of constructing the masonry, and the character of the brick and clay employed, all affect the life of a furnace. The use of furnace blocks, tiles, or ordinary 9-inch fire-brick, each have their advocates; but a furnace lined with the best brick is imperfect unless the clay forming the joints is fully as refractory as the bricks which it unites. If joints are opened, either by the clay being more readily melted or fluxed out, or by not having sufficient non-shrinking material to preserve them, a greater surface of each brick is exposed, and the destruction is consequently more rapid. Excessively thin clay often causes defective joints, owing to the evaporation of the water leaving void spaces.

It is surprising that monolithic hearths and crucibles have not grown in favor in this country. They are employed in other countries, the Swedish practice being to use sandstone or a mica schist ("stallsten"), and place within this a lining composed of crushed quartz, held together by one-eighth to one-tenth of fire-clay, the mass being thoroughly rammed about appropriate forms, so as to give the desired shape and size to the crucible and boshes.

What has been said in reference to the imperfections of fire-brick can be applied with equal force to other metallurgical operations besides the smelting of iron ores in a blast furnace, but this paper has been confined to a few physical features bearing upon the problem. Much has been written as to the chemical composition of refractory materials, and considerable interest was manifested by the Institute in the subject in 1875 and 1876. Mr. Holley, then president, in his address at the Cleveland meeting, in October, 1875, called attention to the necessity of a better refractory material, and showed the cost per ton of product in various works for fire-clay, bricks, etc. A committee was appointed to report upon the subject, and Professor Egleston, as chairman, contributed a paper which covers much of the present knowledge on the subject. This is ably supplemented by the researches of the Geological Survey of New Jersey, and the complete analyses of prominent fire-clays embodied in the special report of Messrs. Cook and Smock. Some experiments to test the refractory character of fire-brick of

various makers were made, under the direction of the Geological Survey of Pennsylvania, in a small shaft furnace, in which it is claimed a heat sufficient to melt steel was maintained. Unfortunately, the experiments were too much restricted, both in time and number, to give reliable results; but, though limited, they are a valuable contribution to our knowledge of fire-brick, as far as their action in the presence of incandescent fuel only is concerned. Although the tests did not not in any case last longer than two hours, all the bricks experimented upon were cut or cracked, some standing much better than others.

The necessity of a better material for metallurgical processes is surely no less important now than in 1875–76, and this paper has been prepared more to revive interest in the subject than to present any novel statement, or to enter into an investigation of the proper chemical and physical composition of fire-brick. The manufacturers of fire-brick have shown a commendable desire to meet the demands of metallurgists as to shape and form, and considerable attention has been bestowed upon composition; but most of these experiments have been limited by individual enterprise and capital. A thorough investigation of the physical and chemical features of fire-brick, in connection with a consideration of the circumstances under which they are to do duty, will undoubtedly prove of immense value to practical metallurgy.

Electric Lights.—In concluding his report upon the different systems of lighting which were exhibited at the Paris Exposition Du Moncel says that the systems of incandescence are already numerous, and doubtless many others will soon be added. Most of them differ from one another only by the composition of the incandescent carbon and its mode of fabrication. It is impossible to base any correct judgment upon the more or less brilliant aspect of the lamps, for, as they are fed by electric generators of very different intensities, those which have the strongest currents naturally furnish the most brilliant and the whitest light. It would therefore be imprudent to pronounce in favor of any single system until a serious examination and comparison has been made by a commission of disinterested scientific men. There is, however, reason to hope for a satisfactory solution of the problem of household illumination by electricity.—*La Lumière Electrique.* C.

SAND FILTRATION AT BERLIN.

By Prof. Wm. Ripley Nichols.

Up to the present time there has been very little done in this country in the way of systematic filtration of water supplies, partly, perhaps, from indifference and lack of information, but mainly on account of the expense. The numerous complaints which arise in the case of almost every city and town supplied with surface water render the question of filtration an important one for the immediate future, and any engineer who has to do with the planning of water works is liable to be called upon to estimate the cost of filtration and to state how far it may be expected to overcome anticipated or existing evils. For this reason, details of the practice in well-managed works, where the conditions are similar to those which obtain with us, are of peculiar interest. Such details are given in a pamphlet recently published by the superintending engineer (Betriebs-Ingenieur) of the Berlin filtration works.* The pamphlet also contains the results of observations on "natural filtration" in the neighborhood and elsewhere.

A very considerable portion of the city of Berlin is still supplied with water from the dirty and sluggish Spree after it has been subjected to a very thorough filtration. The water is taken from the river directly on to the filter-beds, and although the sluggishness of the flow would allow the deposition of much suspended matter, the very considerable water traffic between Berlin and Köpenik, past the water works, keeps the water in a roily condition. Furthermore, the Spree receives, above the point from which the supply is taken, the waste-water of a number of factories, among which are dye- and print-works; it also receives the effluent of a sewage farm mixed, in some cases, with sewage itself. Besides being thus contaminated, the water, especially in time of flood, possesses a deep brownish-yellow color and, at times, a peculiar *pondy* taste due to vegetable extractive matter. Moreover, from spring until fall, a more or less copious growth of *algæ* adds to the disagreeable character of the water.†

* Mittheilungen über natürliche und künstliche Sandfiltration. Nach Betriebsresultaten der Berliner Wasserwerke vor dem Stralauer Thor, bearbeitet von C. Piefke, Betriebs-Ingenieur. 8vo, pp. 75. Berlin, 1881.

† These *algæ* I have found, by personal observation, to be identical with those

The filtration works now supply daily about 40,000 cubic metres (in round numbers $10\frac{1}{2}$ million U. S. gallons) of filtered water—in summer often a larger quantity. There are eleven filter-beds—three covered and eight uncovered—having a total area of 37,000 square metres (about 400,000 square feet). The beds are constructed on the English model, fine sand at the top and coarser material below.* With the large area at disposal it is possible to carry on the filtration at a very slow rate. The water is used, of course, in varying quantities from hour to hour, and, on account of the small size of the clear water reservoir, a constant rate of filtration is impossible; the maximum rate is, however, not over 0·1 metre downward per hour. For the greater part of the time 1 square metre of sand surface is not required to furnish much more than 1 cubic metre of water in twenty-four hours. This would be at the rate of only $24\frac{1}{2}$ U. S. gallons per square foot, and very much less than is the practice elsewhere.† The filtration takes place under a very slight head, seldom of more than 0·5 metre (say 20 inches), but even with this low pressure and slow delivery it has been found impossible, with clean sand alone, to filter the water satisfactorily. If the unfiltered water be allowed to stand a fortnight or so, although the larger of the suspended particles will have settled to the bottom, the water still retains a milky appearance, and sand alone cannot remove the exceedingly fine particles to which this appearance is due. On this account, the water from a freshly cleaned filter is not used at once, but is allowed to stand on the bed and then passed through very slowly until a thin coating has formed on the surface of the sand. This coating is essential to the removal of the finest particles from the water subsequently filtered. Of course, as the coating becomes thicker the filtration becomes more difficult until it partially stops and the filter is "dead."

As has been hinted above, the great trouble in summer is from the abundance of small *algæ* which soon clog the filter. When the *algæ* are absent, a square metre of surface usually filters 20 cubic metres before cleaning is necessary, but in summer the capacity is not over

which occur in our own ponds and which have given much trouble in some of our water supplies, as, for instance, at Springfield and Boston, Mass., and at Albany and New York.

* See Kirkwood, Filtration of River Water. New York: Van Nostrand, 1869. Nichols, Filtration of Potable Water. New York: Van Nostrand, 1879.

† The *maximum* rate, given above by Piefke as 0·1 metre downwards per hour, would be at the rate of nearly 60 U. S. gallons per square foot of surface in 24 hours.

10 cubic metres to the same area. Sometimes the meteorological conditions are such, especially late in the season, that the *algæ* enter into decay and form a peculiar slimy and almost impervious coating over the surface of the sand so that it becomes impossible, under the pressure commonly employed, to pass more than 2 cubic metres of water through 1 square metre of sand surface. This decomposition of the *algæ*, as described by Piefke, has often been noticed in the ponds and reservoirs of the Eastern and Middle States, and the very particular observations which Piefke gives on this point are well worth the the attention of any who are called upon to consider the question of filtration in this country. We may note the fact that, under the conditions just referred to, the superiority of the covered beds became very evident. As has been observed in this country, the decay of *algæ* increased very much the amount of ammonia (of "albuminoid ammonia") and of nitrites and nitrates. A rather curious observation was made in the decomposition of the *algæ* on the open filter-beds. The gaseous products of decay collected in bubbles until these were sufficiently large to break through the slimy coating and rise to the surface. In the hot days of August this evolution of gas was very considerable, and, as a result of this piercing of the coating, a "dead" filter was sometimes brought again into activity; when such a filter was cleaned the little cavities in the sand thus produced were clearly defined, filled, of course, with accumulated dirt.

The *algæ* are not to be regarded as an unmitigated evil. Besides making the removal of the finer portions of the sediment possible,* in their growth they are purifying agents; they change at least a part of the dissolved organic matter into insoluble substances which settle to the bottom and can be removed by filtration. It is, however, desirable that the water should not remain in contact with them after they have begun to decay.

If, after filtration, the apparently perfectly clear Spree water is allowed to stand for eight or ten days, green *algæ* develop themselves, which is probably due to the fact that the spores of the plants are not retained by the filters. This fact, however, is of no practical consequence in the case of the Spree water, for the water is consumed long

*The difficulty, which experiments have shown to exist, in obtaining by sand filtration a clear and bright water from some of our Western and Southern streams, which are charged with clay, is probably greater than it would be in the presence of suspended vegetable matter, which in itself would be considered an objection.

before the spores have had time to develop. This is not the case with all waters, as has been found by experience in the works which supply another portion of Berlin. The "ground water" of the Tegel supply contains an *alga*, the *crenothrix polyspora*, of such rapid development that filtration would not be of much use, as the unremoved spores would develop in the service pipes before the water was consumed.

As has been mentioned, the Spree water often possesses to a marked degree the brownish-yellow color common to streams which flow through marshy or peaty regions and to the water of most impounding reservoirs. This color, with the taste which at times accompanies it, gives rise to general complaint,* but even very slow filtration fails to remove it to any considerable extent. Filters of finely prepared cellulose remove a large proportion of the color, but are hardly practicable on the large scale. If sand filters have a slight action in this direction, it is to be ascribed rather to the sediment containing vegetable fibre than to the sand itself.

A few practical details of management may be of interest. In the Berlin beds the thickness of the sand is 600 millimetres (about 2 feet). At each cleaning the sand is removed for a depth of about 10 millimetres, but fresh sand is not returned to the bed until only about 200 millimetres of the original thickness remain. The foul sand is allowed to stand exposed to the air until, by decay, the organic matter has lost its slimy character; it is then washed and eventually replaced upon the beds. When the filters are emptied the water is drawn completely off, and by successive and systematic stirring nearly the entire thickness of the sand is exposed to the air in order that the small amount of organic matter which was not retained at the surface may be oxidized and destroyed. With the same object in view, the air is allowed to circulate freely, for several days if possible, through the coarser underlying material. The filters are filled from below with filtered water, and then the water is passed through slowly and is allowed to waste for several days.

Artificial filtration is, on the whole, not a difficult operation, but it is one in which the details must be conscientiously and intelligently attended to, and there is no room for negligence or false economy. That the beds must be often cleaned, that from time to time a few thousand cubic metres of filtered water must be used in refilling the beds: these are matters of no consequence if it is thereby possible to

* This, too, is an American experience.

reach the main object in view—namely, the furnishing of a good water. The place for economy is in the choice of the cheapest way of carrying on the various details of the works.

Piefke also describes experiments on the use of iron as a purifying medium as a preliminary to sand filtration, but the experiments did not awaken hopes that iron could be used advantageously on the large scale.

REPORT OF THE COMMITTEE ON SCIENCE AND THE ARTS ON GRISCOM'S ELECTRIC MOTOR AND BATTERY.

HALL OF THE FRANKLIN INSTITUTE,
Philadelphia, January 8th, 1881.

The Sub-committee of the Committee on Science and the Arts, constituted by the Franklin Institute of the State of Pennsylvania, to whom was referred, for examination, Griscom's Electric Motor and Battery, report that a meeting was held at the residence of the inventor, Mr. William W. Griscom, on December 14th, 1880, when the apparatus was exhibited and operated as a motor for an ordinary No. 8 Wheeler & Wilson sewing machine.

A second meeting was held at the Hall of the Franklin Institute, on December 28th, at which time two motors were carefully examined by the committee.

A number of queries in relation to the novel features claimed for the apparatus, and their possible practical value, had been submitted to the inventor; these, together with his replies thereto, were read and considered.

Members of the committee have also informally visited the laboratory of the inventor for the purpose of witnessing the operation of the motor under varying conditions, and to make such tests as lay within their power.

The mechanism may be described as follows:

The motor consists, briefly, of two semi-circular electro-magnets which together form a ring; their poles project inward and, together with the wire coils, form a cylindrical tube within which a Siemens armature revolves.

The poles extend laterally beyond the ring, forming supports for the brackets which carry the bearings of the armature and the brushes of the commutator. In order to reduce the wear of the journals to a

minimum, the bearings are made four times the diameter of the shaft, and the direction of the wear is away from the point of nearest approach, so that the poles of the armature and magnets can never come in contact from this cause; a frequent source of annoyance and danger in former motors.

The friction wheels of the brushes are in pairs and the shape of the commutator is such that one wheel will always touch one-half of the commutator before its companion leaves the other.

The tension of the belt is readily adjusted by means of a fork which carries the motors.

The battery consists of six one-gallon cells, into each of which plunges a plate of zinc 4 inches long and 2 inches wide and two plates of carbon exposing a like surface.

The large amount of liquid (electropoion) is merely to save the trouble of frequently recharging; a battery containing six drachms per cell gives equal power but for a shorter period. It is estimated that the battery once charged will continue to supply the motor with sufficient power for all ordinary use of a sewing machine in a private family for many months, or probably one year, without refilling. It is inclosed in a tight box which, covered with a cushion, serves as a seat for the operator. The method of automatically removing the plates from the solution when not in use will be found described elsewhere in this report.

In the course of this investigation, certain seemingly novel and certainly interesting theories relating to this subject were elicited from the inventor; but, as this report is designed to be a simple statement of observed facts, the committee would recommend that a special committee of experts in electrical science be appointed to thoroughly explore and decide upon the merits of these purely theoretical considerations.

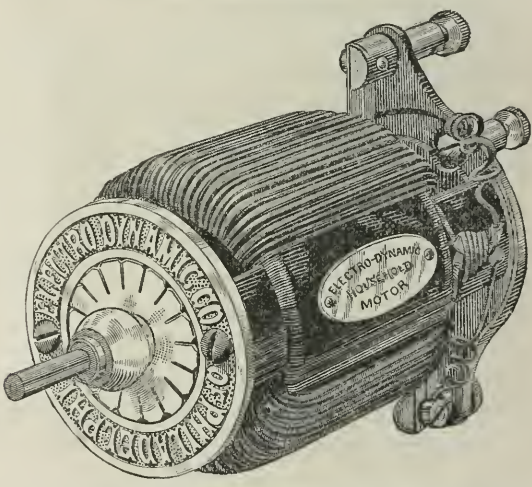
The inventor, in his application (No. 1188) dated November 23d, 1880, to the Committee on Science and the Arts of the Franklin Institute, wrote as follows:

"Gentlemen.—You are respectfully requested to examine into the merits of and report upon Griscom's Electric Motor and Battery. Your attention being more especially invited to the power of the motor in proportion to its size, its simplicity of construction, its mechanical details and its adaptability to general use, and also to the method of

graduating the current from the battery and other peculiarities, rendering it a safe and suitable battery for household use."

The committee, having made a careful examination of all of these points, begs leave to report upon them *seriatim* as briefly as possible.

First. Power.—The power of the motor depends upon the quantity of electricity furnished by the battery; this is easily regulated by raising or lowering the zinc and carbon plates in the exciting fluid. It was found that when the plates were but partially plunged in the bath, sufficient mechanical power was developed by the motor for all ordinary requirements of a sewing machine, and when fully immersed it was more than sufficient to drive a large needle through sixteen layers of cotton cloth at a very rapid rate.

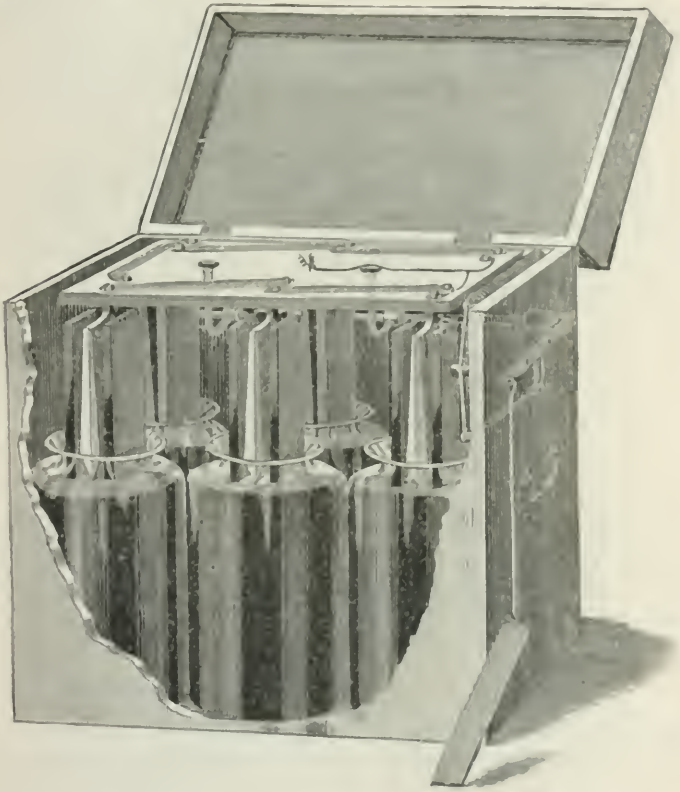


Second. Size.—The size of the motor is $2\frac{1}{4}$ inches in diameter and 4 inches long and its weight is but $2\frac{1}{2}$ pounds; it is securely attached by a light frame to the table of the sewing machine.

Third. Construction.—The entire apparatus appears to be simple in its construction, excellent in all its mechanical details, and its adaptability to general use is not questioned by the committee.

Fourth. The Battery.—This differs from the ordinary Grenet form mainly in the automatic arrangement for removing the plates from the bath and in the large size of the cells, holding one gallon of "electropoion" fluid each. The method of graduating the strength of the current, and consequent speed of the motor, is as simple as it is effective. A very slight pressure of the foot on the treadle suffices to start

the machine as gradually as may be desired; the speed may then be increased up to one thousand or more stitches per minute which, it is said, is considerably faster than is now attained by professional sewing women, while others seldom sew more than 300 to 400 stitches per minute.



Two forms of the battery were shown, in both of which the plates are automatically raised above the bath when not in actual use. In one form this is accomplished by means of a spiral spring attached at either end of the bar to which the plates are permanently fastened. In the other a similar result is attained by means of a counter weight on the small arm of the lever attached to the treadle.

It would seem to your committee that the important novel feature in this battery consists in the size of the cells, which thus enables it to continue operative, without recharging, for a great length of time. As the current is necessarily intermittent when the motor is running, and

as the plates are frequently raised and lowered by the operator to accommodate the needs of the work of sewing, the main objection to the ordinary Grenet battery, viz., the rapid deterioration when a constant current is required, is avoided to a great extent, while its advantages for household and occasional use are retained. These are as follows: It generates no gases or vapors "that are practically deleterious;" the zinc elements do not, as in other batteries, require frequent amalgamation or attention, and when not in use are simply raised above the fluid and allowed to drain.

In conclusion, the committee recomends "Griscom's Electric Motor and Battery" to the favorable consideration of the Franklin Institute as an apparatus possessing great power in proportion to its size, simplicity in its construction, excellence in its mechanical details and general adaptability to household use.

ALEX. E. OUTERBRIDGE, *Chairman.*
E. ALEX. SCOTT.
ROBT. B. HAINES, JR.
ADDISON B. BURK
N. H. EDGERTON.

At a stated meeting of the Board of Managers of the Franklin Institute held November 9, 1881, the Elliot-Cresson Gold Medal was awarded to William Woodnut Griscom for his invention of the Electric Motor, upon the recommendation of the Committee on Science and the Arts.

Various Effects of Magnetic Armatures.—In 1863 Hughes undertook a series of experiments which demonstrated the advantage of armatures for the electro-magnets which he employed in his printing telegraph, but the question was not studied generally. Du Moncel has therefore undertaken a variety of experiments, and he has found that the effects may be diametrically opposite, according as the magnets act only by one pole or by both. In the former case there is generally a disadvantage, in the latter an advantage. The effects also vary with the relations which the armatures bear to the inter-polar interval. In an ordinary electro-magnet, with two armatures, which may be placed at any required distance from each other, the greatest effect is generally found when that distance is about one-fourth of the inter-polar distance.—*La Lumière Electrique.* C.

THE SCIENTIFIC PRINCIPLES INVOLVED IN ELECTRIC LIGHTING.

By Prof. W. Grylls Adams, F.R.S.

A series of "Cantor Lectures" delivered before the Society of Arts, London, 1881

(Continued from page 375.)

With Gramme Machine.

In Auerbach and Meyer's experiments for 800 revolutions a minute, the maximum electro-motive force is 76 volts and for 51 volts, or two-thirds of the maximum value, there is a current of 6·5 webers through a resistance of 7·8 ohms. Below this value the current is unsteady. With Siemens' machine, a speed of 700 revolutions a minute gave a maximum electro-motive force of 76 volts, and for 51 volts there is a current of 15 webers through a resistance of ·6654 ohms. With a small Siemens machine, a speed of 1000 revolutions per minute gave a maximum electro-motive force of 42 volts, and for two-thirds of this, or 28 volts, the current was 11·2 webers through about 2·2 ohms resistance.

Dr. Hopkinson has investigated the way in which the electro-motive force in a Siemens machine depends on the current. He has shown that:

1. The electro-motive force is, for a given current, proportional to the speed of revolution of the armature.

2. That the electro-motive force does not increase indefinitely with increasing current, but,

3. Only increases in the direct ratio as the current increases up to about two-thirds of its maximum value.

The current is very unstable for small change of resistance, or of speed of engine, as long as the value of electro-motive force is less than two-thirds of its maximum value. There is a remarkable difference in the ratio $\frac{E}{C}$, depending on change of speed from 600 to 700 revolutions a minute, where the current changes from 5 to 15 webers, for this increase of one-tenth of the speed.

As regards the relation of work converted into electrical energy to

the work expended to produce it, it appears from the experiments of Mr. Schwendler and Dr. Hopkinson that, with the Siemens machines employed by them, the loss of power was from 12 to 14 per cent., so that if the external resistance of the circuit, *i. e.*, the electric lamp, etc., be so adjusted that half the total work produced appears in the arc, then 43 or 44 per cent. of the total work expended is produced in the arc.

The results arrived at by Dr. Siemens, with his latest machine on Wheatstone's principle are: 1. That the electro-motive force, instead of diminishing with increased resistance, increases at first rapidly and then more slowly towards an asymptote. 2. That the current in the outer circuit is actually greater for a resistance of $1\frac{1}{2}$ ohm than for one ohm.

With a current of 30 or 40 webers, the horse-power expended was 2·44 h. p., and the effective work 1·29 h. p., giving an efficiency of 53 per cent., as compared with 45 per cent. in the ordinary Siemens machine. The maximum energy which can be converted into heat in the machine is 1·3 h. p. The new machine will give a steadier light with greater economy, and may be driven by a smaller engine.

The Brush Machine.

Among the latest continuous-current machines are two which promise to be very successful machines. The Brush, with a ring on the Gramme system, with eight divisions or portions hollowed out to receive the coils, the bobbins at opposite ends of a diameter being connected together and to a commutator. When a pair of bobbins passes the neutral point, so that there is no current in it, it is put out of circuit for one-eighth of a revolution, so that the current produced in the other bobbins is not wasted, by being sent through the resistance of the two which are producing no current. On the inducing magnets are wound fine wires, offering considerable resistance, which carry the current when the external circuit is open and keep up the magnetism; but when the circuit is closed, the thick wires on the magnets carry the principal part of the current.

The internal resistance of the machine being about $10\frac{1}{2}$ ohms and the external resistance 73 ohms, there was, according to calculation, a current of 10 webers and an electro-motive force of 839 volts. With these numbers, the effective work on the external circuit ought to be

87·36 of the whole electrical work produced; but, practically, it is only 61 per cent.

This relation of work converted into electricity to the work expended in this machine, is about 73 per cent., whereas with both Gramme's and Siemens' machines, with relatively smaller external resistances, this ratio is about 88 per cent.

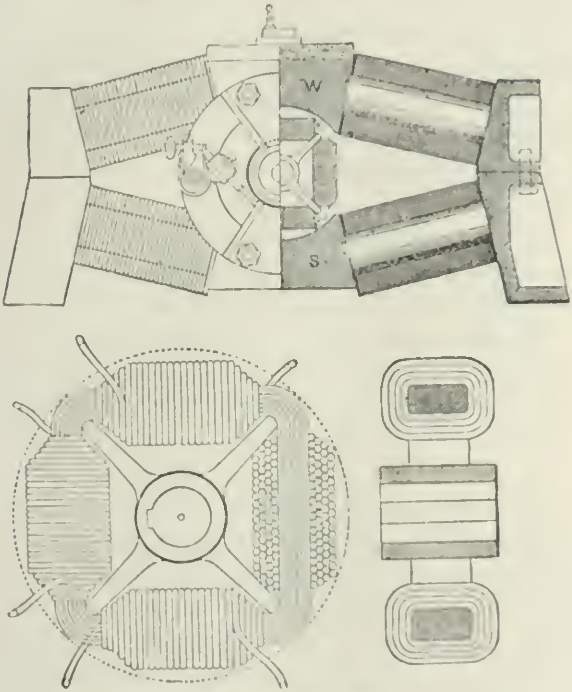


FIG. 5. The Bürgin Machine.

Another continuous-current machine is the Bürgin machine, from Switzerland, which has only just been introduced into England by Mr. Crompton. Four or six coils are wound on the sides of a square or hexagonal frame, consisting of iron wires. The corners of the frame come very near to the poles of the magnets. There are six or eight of these frames arranged successively in the form of a helix. The action is similar to that of the Gramme machine, the dynamo-electric principle being introduced in this as in other machines. The construction of the machine is very simple, and its efficiency has been proved by M. du Moncel and also by Mr. Crompton to be remarkably good. These machines are of small internal resistance, and are driven

at high speeds (up to 1600 revolutions a minute), so that there is considerable electro-motive force.

The efficiency of certain Gramme machines, exhibited by Mr. Crompton and tested at the Glasgow Electric Light Exhibition, was shown to be such that, with a power of 4 h. p. expended in producing the current, only $\frac{1}{2}$ h. p. was expended on friction and passive resistances, so that about 88 per cent. was net power. This $3\frac{1}{2}$ h. p. converted into electricity gave a current of 32 webers through a resistance of about 2 ohms, *i. e.*, an internal resistance of 1·077 ohms, and the arc of a Crompton lamp giving a light equivalent to 2158 candles.

Now, we may compare with these the results obtained by Mr. Crompton for the Bürgin machine, running at a speed of 1675 revolutions per minute.

Five machines were tested, and the total work expended was 5·45 h. p. The amount spent on friction and passive resistances, when the circuit was open, was about ·25 h. p., so that about 86 per cent. is net power. The work converted into electrical energy, 5·2 h. p., gave a current of 20·15 webers through an internal resistance and conducting wires of 2·8 ohms, together with the arcs of 3 Crompton lamps (about 5 ohms), each giving a light of 2103 candles, measured horizontally; the electro-motive force $=\frac{\text{work}}{\text{current}}$ being equivalent to 163 volts.

With photometric measurements made horizontally, the electric light being level with the gaslight, the carbons being concentrically adjusted, and the length of the arc being about 3 m.m., the greatest amount of light was found to be obtained at 1675 revolutions per minute, with 3 lamps, each of 2103 candles, or with 4 lamps, each of 1246 candles. The upper carbon was 10 m.m. and the negative carbon 13 m.m. in thickness. The consumption of the upper carbon was 4 c.m. and the lower nearly 2 c.m. per hour. The total horsepower expended was 5·55 h. p., and the current, with 3 lamps, varied from 18·36 to 21·94 webers, and with 4 lamps, from 16·9 to 19·6 webers. All three lights were very steady and much whiter than the single lights of Gramme's machine.

Mr. Crompton has been kind enough to lend me, this evening, a new Bürgin machine, about which he gives me the following facts: It was tried at 1620 revolutions a minute, and a current of 28 webers was sent by it through 3 lamps, in series. When the arcs were lengthened to one-fourth of an inch each, the current was 24 webers,

and the arcs gave a light of 5000 candles each, the photometric measurements being made in the most advantageous direction.

The British Electric Light Company have been good enough to place at my disposal, for this evening and for my lecture next week, two Gramme machines for trying some of the electric lamps which have been kindly lent to me.

These machines are driven by a steam engine lent by Messrs. Robey, of Lincoln, and for the Brockie and other electric lamps I am indebted again to the British Electric Light Company, to Dr. Siemens, to Mr. Crompton; to Mr. Latimer Clark for the Lontin lamp; for the Rapieff and Wilde electric candle, to Mr. Berly; to the Jablochkoff Electric Light Company for their candles; and to the Anglo-American Light Company for the Brush lamp.

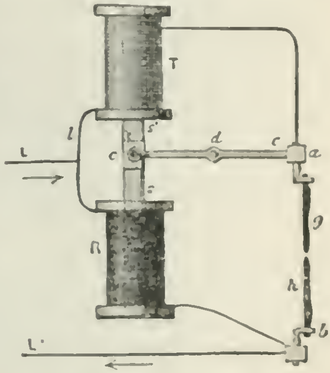


Fig. 6.—Siemens' Differential Lamp.

THE BROCKIE LAMP.

The upper carbon is attached to an iron tube, which passes into a solenoid, through which it passes as the positive carbon burns away. The solenoid forms a shunt or by-pass for the arc, and takes a small part of the current and holds up the iron tube which carries the upper carbon; as more current passes through the coils, the motion of the carbon is stopped.

A commutator is so arranged and driven by the dynamo machine as to break the current and allow the carbons to come in contact for an instant at regular intervals, say every minute. Then the circuit is completed again, the upper carbon is drawn to its proper distance apart, and the light continues. At every minute the light goes out, but instantly relights, and no variation of light is perceived.

SIEMENS' DIFFERENTIAL LAMP.

A thick-wire bobbin (T) carries the arc current, and another fine-wire bobbin (B) forms a shunt to the arc. The interval between the bobbins equals the height of each of them. The iron rod *s s'* is of twice the length of each bobbin, and its ends in the normal position are at the centres of the bobbins. The attraction by the thick-wire

bobbin tends to lengthen the arc and diminish the current, and so its attraction is weakened and the arc is again diminished, the attraction on the iron being regulated by the change of resistance in the arc. A pendulum arrangement is attached to prevent the oscillations of the carbon from being too sudden.

Crompton Lamp.

The carbons are brought together by means of the weight of the upper carbon holder, as in the Serrin lamps. The carbons are controlled by means of an electro-magnet, of which the principal armature separates the carbons, and a light secondary armature is arranged on the back of the large one, and does the more delicate work of bringing the carbons together. The large armature supports the negative or lower carbon; and when the small armature has brought the carbons together, so that a current passes, the large armature separates them to the proper distance apart for a good light. When the arc is broken, the armature, supported by a spring, is raised, and brings the carbons into contact, and relights the lamp. The small variations in the strength of current react on the second armature, which is held at some distance above the large armature by a light spiral spring. The small armature carries an arm, which is applied as a brake wheel, which is the last wheel of a train of wheels set in motion by the weight of the positive rod.

Regulator in Brush System.

A very pretty arrangement for shunting the current past a lamp (when it is not in use), so that one lamp may be put out without affecting the other lamps in the circuit, is adopted on the Brush system.

The current passes through a solenoid coil, wound with thick wire, and then passes to the upper carbon, through the arc to the lower carbon, and then by the frame to the next lamp. The solenoid holds up a rod of iron, which tilts a ring on one side, through which the carbon passes, and so locks it. To the end of a thick wire of the solenoid is attached a thin wire (150 ohms), which is also wound on the solenoid, and which forms a shunt or by-pass to the arc, taking more and more of the current as the resistance of the arc increases. This thin wire is wound the opposite way, and the current in it relaxes the hold on the carbon, so that it falls slowly, and then takes more of the current. As soon as it does so it is again held fast. To prevent

the carbon from falling too rapidly it is passed through a vessel containing glycerine, and slides downwards very slowly. The current through the thin wire also passes through another solenoid, which forms a shunt or by-pass to the whole lamp, so as to take all the current past the lamp if it should get out of order. When a considerable current flows by this path—*i. e.*, if the arc becomes an inch long, so that its resistance is greatly increased—the second solenoid draws up a piece of iron, which lets all the current pass, and the lamp is thrown out of the circuit.

In the Brush lamp, which is designed to burn 16 hours, there are two pairs of carbons, with the rings on the upper carbons, which hold them by friction, so adjusted that one is held about one-fourth of an inch above the other, and, therefore, the second carbon will not come into action until the first falls or is burnt out.

All the electric candles, such as the Jablochkoff candle, the Jamin candle, the Wilde candle and the De Meritens candle, consisting of three carbons, are fed by means of alternate current machines, because it is essential that the two carbons should burn away equally. In the Jamin and the Wilde candle the carbons are at first in contact, but when the current passes one of the carbons is separated from the other, because its holder is set on a hinge, so as to be acted upon by a small electro-magnet through which the current passes.

M. Joubert has found that it is necessary, in order to keep the arc steady with the Jablochkoff candle, that the alternate current in the circuit should have a mean value of eight or nine webers, and that below five webers the arc cannot be kept alight; between the bases of the two carbons forming the candle there is an electromotive force of 40 or 45 volts. The Jablochkoff candle uses up about 66 kilogrammetres of work, of which 33 kilogrammetres, or 4·6 h. p. is converted into heat and light.

When the arc is produced in a magnetic field, either by disturbing it by an electro-magnet, or by placing a frame around it, as in the Jamin candle, it is necessary to have a current half as large again as when the electro-magnet is not in action. One-third of the energy of the current is in such a case spent in producing a strong magnetic field around the electric arc, and is, therefore, so much wasted energy, as far as the electric light is concerned.

When gas was first introduced extensively for lighting purposes, many objections were raised to its use, and among them was one which

was recorded by Clement Desormes, in 1819, which is summed up in the following quotation:

"The light is of a disagreeable yellow color, entirely different from that red and warm gleam of oil lamps; it is of a dazzling brightness; its distribution will be impossible and irregular, and it will be much dearer than oil lighting, and, even if it should be improved, it will still remain much dearer than those lights which we already possess."

Just as Desormes had become accustomed to the red gleam of oil lamps, and objected to the coldness of the yellow gas light, so, a year or two ago, a similar objection was raised against the electric light, that it was entirely different from the yellow and warm gleam of gas-light; that it is of a dazzling brightness; that its distribution would be impossible and irregular; and that our streets would be left in darkness.

These objections do not seem to be so strongly taken up by the public as they were two years ago, for they have seen several trials of the electric light; and, although there are many difficulties in the way, yet the fact that the electric light has all the colors more uniformly blended, and is, therefore, a whiter light than gas, and enables objects to be seen in their true colors, can hardly be urged any longer as an argument against its use. The same argument might be urged for the same reason against bright moonlight, or against the light of day, and in favor of the yellow London fog. The Kyrle Society, in its search after truth and beauty, must surely be strong supporters of the spread of the electric light.

If we return to the Report of the House of Commons, we find the following statement:

"A remarkable feature of the electric light is that it produces a transformation of energy in a singularly complete manner. Thus the energy of 1-horse power may be converted into gaslight, and yield a luminosity equal to 12-candle power. But the same amount of energy transformed into electric light produces 1600-candle power."

The experiments of Mr. Schwendler, of Dr. Hopkinson, and of others, have shown that, both with the Siemens machine and with the Gramme machine, 88 per cent. of the total work expended is converted into electrical energy. Theory has established that, if the external resistance of the circuit is equal to the internal resistance of the battery or magneto-machine, the available work in the external circuit is a maximum.

Suppose, then, that we have 40 Grove's cells, each of ·25 ohms resistance, and of an electro-motive force of 2 volts, the external resistance being 10 ohms—

$$\text{Then } Q = \frac{E}{R + r} = \frac{40\,E}{40 \times \cdot 25 + 10} = 4 \text{ webers,}$$

$$\text{and } EQ = 2 \times 4 \times 40 = 320.$$

The work done in the external circuit is $\dfrac{320}{9{\cdot}81 \times 2} = 16$ kilogram-metres per second nearly, or about $\frac{2}{9}$ h.p.

A small Gramme machine of the A type, having an internal resistance of 4·58 ohms, and with an external resistance of 4 ohms, gives an electric current of 17·5 webers and an electro-motive force of 158·5 volts, giving an amount of work equivalent to 2 h.p., $EQ = 160 \times 17$ nearly = 8 times the energy of 40 cells of Grove.

If we wished to replace such a machine by Grove's cells, we should have to arrange about 80 cells to get the same electro-motive force, and to make each cell about four times as large, or to arrange 320 cells in four sets of 80 in each set, to get the same amount of external work done as by the Gramme machine. This will show how impossible it is to do the work by voltaic batteries which can be done by magneto-electric machines.

The equation, work = EQ, may be satisfied in two ways—either by making Q large and E small, *i. e.*, making what is called a quantity machine which will only do effective work when the external resistance is small; or we may make Q small and E large, *i. e.*, what is called a tension machine, which requires an external resistance large enough to prevent the machine from being overheated, and to satisfy the relation for the greatest amount of external effective work.

Comparison of Two Gramme Machines.

	Quantity.	Tension.
Number of turns per minute, .	797	967
Internal resistance, . .	1·2	4·58
External resistance, . .	1·14	4·00
Current in webers, . .	29·67	17.51
Electro-motive force in volts, .	81·58	158·50
Work spent to produce current, .	243	277

Thus we see that the total amount of energy is nearly the same in the two cases, but in one case it is spent in driving a large current through

a small resistance, and in the other a smaller current is sent through nearly four times the resistance, and to do this a higher electro-motive force is required. This higher electro-motive force is obtained by increasing the number of turns of wire in the bobbin and in the magnet, so strengthening the magnetic field, and also by increasing the number of turns of the machine.

We arrive, then, at the conclusion that, to overcome higher resistances more effectually, higher electro-motive force, and therefore higher speed is required. Now our resistances may be so high that an ordinary current of electricity, even from a dynamo-machine, will not pass through it, in which case we have to resort to another method of producing electricity, of still higher electro-motive force, but the quantity produced is then considerably diminished. We have then to take an induction coil, consisting of two coils, in one of which a current of electricity from a battery is passing, and by suddenly breaking and making this current, to obtain great changes of the magnetic field, and hence great electro-motive forces, and so get very powerful alternating currents. We know the effect of checking suddenly the flow of water in a pipe. Sometimes the increase of pressure so produced may be sufficient to burst the pipe, and this is one transformation of the energy of motion of the water. This is analogous to the development of the energy of the induction current by the sudden checking of the electric current in the primary circuit. Water may be raised to a high level by a series of sudden impulses, as in the hydraulic ram. A flow of a considerable quantity of water being suddenly stopped, there is at once a sudden increase of pressure, which is sufficient to lift a valve, and allow a small quantity of water to pass into the reservoir or air-chamber. This air-chamber regulates the action of the flow of water up the pipe from the reservoir, just as the resistance and capacity of the secondary circuit regulate the secondary induction current when the primary current is broken. The action of the induction coil is very well illustrated by the action of the hydraulic ram, the level to which water is raised corresponding to the electro-motive force of the secondary circuit. Just as in the hydraulic ram, the quantity of water raised by the machine is at the best only about 66 per cent. of the quantity used, so in making use of the induction current to do work, or to produce the electric light, it is impossible to convert more than a fraction of the energy of the original current into useful work.

In the two systems of electric lighting to which I wish to draw

special attention this evening we have instances of the two opposite methods of accomplishing the same end, viz., the lighting of moderate-sized rooms by a steady and pleasant light.

The Werdermann or Joel Electric Light.

In the Werdermann system, or the Reynier system, a small thread or point of carbon abuts against a plate or edge of carbon or of copper, and becomes heated by the current so as to give out a glowing light, and gradually consumes away, but more and more slowly as the carbons are are more and more improved. In these lamps, kindly lent to me by Mr Latimer Clark, and in these Joel lamps kindly lent to me by Mr. Joel, who has introduced several improvements into the original Werdermann lamp, the resistance of the contact of carbon is very small, about ·134 of an ohm; hence it will take several of them, 7 or 8 (or perhaps 10), arranged in series in the same circuit, to equal the resistance of the electric arc. To work these lamps of low resistance only a low electro-motive force is required, and so the result is attained by driving a small resistance dynamo-electric machine at moderately low speed; or by placing a considerable number of lamps in series, so as to make their combined resistance equal to or greater than the internal resistance of the machine. Thus a Gramme machine, revolving at the rate of 1200 revolutions a minute, giving an electromotive force of about 130 volts, will give a current of 50 webers through about 10 lamps in series. But this current gives an illumination of 320 candles in each lamp, so that with this current we get an illumination of 3200 candles in 10 lights. Now, the energy expended to produce this rate of revolution in a Gramme machine is about 9 or 10 h.p. Hence the Werdermann, or the Joel lamp, gives at least two lights of 160 candles each for each h.p. of energy expended.

Mr. Alex. Siemens lays down, in his paper on "Electric Lighting," that 4 lbs. of coal, costing 15s. a ton, will produce 1 h.p. of energy per hour, and that, if a steam-engine be employed to produce an electric light of 6000-candles power, the cost would be 5d. per hour. If the same illumination be produced by 15 lights of 400 candles each, the cost would be 2s. 1d., or five times as much. Hence the cost for a 400-candle light would be at the rate of about 1⅔d. per hour.

Now, by comparison, we may get some idea of the price of the electric light when obtained by means of the Werdermann or Joel lamp. If we compare the light obtained by the Joel or Werdermann lamp

with that from the 400-candle light from the arc, we get about 320—or, say, 300—candle power in the Joel light for 800-candle power in the other. Hence the price of the electric light from a Joel lamp should be at the rate of 6½d. per hour for a 600-candle power light.

Now, according to Mr. Alex. Siemens' estimate for gas, the price of gas would be at the rate of about 5⅘d.—or nearly 6d.—per hour for the same light. In other words, the cost of the electric light from the Joel lamp would be nearly the same as gas at the rate of 4s. per 1000 cubic feet.

In estimating the candle-power of lamps it is usual to place the photometer on the same level with the lamp, so that the surface is illuminated by the rays proceeding horizontally from the lamp. Now, in all lamps, whether Werdermann or arc lights, which are fed by a continuous current machine, the current passes from the positive carbon to the negative always in the same direction; and in the arc lights, the upper positive carbon becomes worn away into a hollow; hence a portion of this carbon obstructs the light, and the greatest intensity of light is not in a horizontal direction, but downwards, at an angle of about 60° below the horizontal. The illumination in this direction is about three times—or even more than three times—the illumination in the same horizontal plane with the arc; hence, when it is said, in the report of the Glasgow tests, that a dynamo-machine, at 1200 revolutions per minute, will give a light of 2060 candles, for an expenditure of 4 h.p.—the light being measured horizontally—we see that the illumination, in a direction inclined downwards at an angle of 60° below the horizon, would be 6500 candles for 4 h.p. or at least 1625 candles per h.p. This will also explain why lights fed from continuous-current machines should be placed at a considerable height above the area to be illuminated. This, combined with the fact that it is far more economical to produce one very powerful light by means of a large machine, than several smaller lights to illuminate the same area to the same degree, will explain why Dr. Siemens is erecting his large lamps at so great a height, for the trials of electric lights which we shall shortly have an opportunity of seeing in the city.

Sub-division of the Electric Current.

The next point to which I propose to draw your attention this evening is the sub-division of the electric current.

It will be simplest to regard first the case where there is a battery of given electro-motive force. In this case, according to Ohm's law,

$$E = C(R + r),$$

where E is the electro-motive force, C the current, R the resistance of the battery, and r the external resistance. If the poles of the battery be joined by two separate resistances, r_1 and r_2,

$$\text{then } E = C\left(R + \frac{r_1 \; r_2}{r_1 + r_2}\right).$$

If the resistance of each branch is equal to r, and if C_1 be the current in each,

$$\text{then } E = C(R + r) = 2\,C_1\left(R + \frac{r}{2}\right).$$

Let $E = 100$ volts, $R = 1$ ohm, and $r = 100$ ohms,

$$\text{then } 100 = C(1 + 100) = 101\ C,$$

$$\text{and } 101 = 2\ C_1\,(1 + 50) = 102\ C_1.$$

Hence nearly the same current flows in each branch as when there is only one wire. If there are 10 branches instead of 2 branches, and if C_x be the current in each,

$$\text{then } 100 = C_x\,(1 + 10) = 110\ C_x,$$

i. e., the current in each branch is $\frac{100}{110}$ instead of $\frac{100}{102}$. If there are 50 branches, and C_y be the current in each,

$$\text{then } 100 = C_y\,(1 + 2) = 150\ C_y,$$

thus the current in each is $\frac{100}{150}$ or $\frac{2}{3}$, and the heating or glowing effect is $\frac{4}{9}$ of it value with only one branch.

Now, if with 50 branches in multiple arc, we diminish the external resistance of each branch so as to get the same current as at first through each branch,

$$\text{then } E = C(R + r) \text{ at first,}$$

$$\text{and } E = 50\ C\left(R + \frac{r_1}{50}\right) \text{ with 50 branches.}$$

$$\text{So that } (R + r) = 50\ R + r_1.$$

$$\text{Or } r - r_1 = 49\ R.$$

Hence with $R = 1$ and $r = 100$ ohms $r - r_1 = 49$ and the length left has a resistance of 51 ohms, the heating of each of these is $\frac{51}{100}$, or one half of what it was with only one branch. Hence the glowing heat or light from such a resistance will be greater than from the unshortened wire, with the weaker current through it. In this case we get 50 circuits of 51 ohms each, so arranged that the heating effect in each circuit is ·51, or about one-half of what it was at first. Hence

the amount of heat radiated from each is one half of what it was at first. But there are 50 such circuits, therefore the total heat radiated is 25 times as much as it was with only one branch.

If the resistance of the battery and connecting wires is considerable, then we see that the addition of every additional branch circuit takes away greatly from the amount of heat radiated from each branch, so that this plan of sub-division by separate circuits can only be adopted with success when the internal resistance is small as compared with the external resistance. We see, then, that with small internal resistance there is great gain in heating, and, therefore, in light-giving power, by arranging branch parallel circuits in multiple arc; but when the resistance of the battery and leading wires is considerable, the advantage of this arrangement is small, and very little sub-division is admissible.

INCANDESCENT LAMPS.

Now, let us consider the case of currents produced by means of dynamo-electric machines, in which the electro-motive force is not constant in the same machine for the same speed, but depends upon the resistance of the circuit. An electro-motive force of 100 volts produces a current of one weber through a resistance of 100 ohms, and Mr. Swan tells us that this current, through a lamp of that resistance, gives a 60-power candle light. Now, if we reduce the length of the carbon filament in the lamp without altering the current, we reduce the illuminating power in the same ratio. Suppose we take it as four-fifths of the length, *i. e.*, its resistance is then 80 ohms, and we shall get a 48-candle power light from the same current (one weber), *i. e.*, with an electro-motive force of 80 volts.

With two such lamps in series we shall get two 48-candle power lights, with an electro-motive force of 160 volts, sending a current of one weber through them, *i. e.*, the two lamps should give out a light of six gas-burners of 16-candle power each, and should be sufficient to illuminate a drawing-room in many of our London houses.

If we consider now how we are to produce this current, we find that a Bürgin machine, by the expenditure of 6 h.p., will send a current of 24 webers through an external resistance of about 7 ohms, giving an electro-motive force of 160 volts. If then we take two lamps in series, *i. e.*, 160 ohms, and arrange 24 distinct series, we shall get a combined resistance of $\frac{160}{24}$, or about 7 ohms, allowing for the resistance of connecting wires, and there will be a current of 1 weber through each cir-

cuit, *i. e.*, this machine should give us 18 lights each of 48-candle power. With a resistance of 50 ohms in each lamp, the number of lamps which may be supplied from the same machine will be double this number. If we reduce our electro-motive force from 100 volts to 80 volts, with the same length of carbon in the lamp, then we reduce the current from 1 weber to $\frac{8}{10}$ of a weber. This in the same resistance will reduce the illuminating power from 60 candles to a light of about 40-candle power, instead of a light of 48-candle power. Hence, with a given electro-motive force, more light is obtained, and, therefore, greater economy is effected by shortening the length of the carbon in the lamp, rather than by diminishing the current through the same length of carbon. Hence, the best results will be obtained in incandescent lamps by sending through them as strong a current as they will safely stand, and making the length of carbon such that the dynamo-machine employed will send such a current through them.

Take another case: Suppose we have one lamp of 75 ohms resistance (*i. e.*, about 45-candle power). A Gramme machine or a Siemens' medium-sized machine will give an electro-motive force of 100 volts, and a current of about 25 webers, at the rate of 100 revolutions a minute, through an external resistance of about 3 ohms. Hence, if we have 25 lamps in separate branch circuits, or in multiple arc, we get 1 weber through each from such a machine, and get a light, according to Mr. Swan, of 45-candle power from each. Hence, such a machine will give us about 1125-candle power illumination. The energy expended would be about 5 or 6 h.p., so that the illumination would be about 200 candles per h.p.

We have seen above that, with the Siemens' alternate current machine, a 400-candle light requires about half a horse power; so that 1 h.p. will supply two lights of 400-candle power, from an alternate current machine at the rate of 10d. for 3 hours. The same illumination can be obtained from gas at 2s. for 3 hours. Now, two-thirds of this cost is for the supply of carbon, which becomes burnt in in the arc. Hence, without this consumption of carbon, the expense per h.p. is only $\frac{10}{9}$ of 1d. per hour. Applying this to the case of incandescent lamps, in which our carbons do not wear out, we see that by a proper arrangement of the lamps we may get a 200-candle power light at the rate of $\frac{10}{9}$ths of 1d. per hour.

Now, Mr. Alex. Siemens also states, in his paper, that at the rate

of 3s. 6d. per 1000 feet, the same illumination cannot be obtained from gas at less than 2d. per hour. Hence, allowing $\frac{8}{9}$ of 1d. an hour for the breakage of incandescent lamps, the cost of light by gas and by incandescent electric lamps would be nearly the same.

If we allow that only a light of 40-candle power, instead of 60-candle power, can be produced at this rate, still the incandescent light cannot be regarded as an expensive light.

Now, in the absence of any actual determination, let us assume the same law to hold in the Brush system as in the Siemens or the Gramme system. In the Brush system a current of 10 webers is sent through an internal resistance of 10 ohms, and an external resistance of 70 ohms. Now, in the Siemens machine, when the external resistance is seven times the internal resistance, the current is only $\frac{1}{60}$th part of its value when the external and internal resistances are equal, or $\frac{1}{40}$ of its value when the external is double the internal resistance. The drawback to this arrangement would be that one-third of the total work expended would be lost in heating the machine.

Taking the Brush machine as worked at present, the difference of potential for each of 16 lamps in circuit is about 40 volts. Hence total difference of potential of 16 lamps = about 640 volts. With an external resistance of 70 ohms there is a current of 10 webers. Hence, if we arrange incandescent lamps in 10 series, so as to get a resistance of 70 ohms, we shall get 1 weber through each series. Put, then, 7 lamps, each of 100 ohms resistance, in each series, and we shall get 70 lamps from a Brush machine. These 70 lamps are each of 60-candle power, and all are worked by an expenditure of 16 h.p. Hence the candle power is 4200 candles from 16 h.p., or 262·5 candle power per h.p. If the lamps of this resistance are only heated, so as to give a light of 30-candle power each, then the candle power per h.p. will have to be reduced.

Thus we have seen that it is possible to sub-divide the electric current in such a way as greatly to increase the amount of illumination which may be obtained by means of a dynamo-electric machine, especially when the light is accomplished by the incandescent system of Swan, Lane-Fox, or Edison.

The earliest attempt to obtain light by incandescence in a vacuum was made by King, in 1843, who applied continuous metallic and carbon conductors, and heated them by an electric current in a Torricellian vacuum. He was followed in 1848 by Staite, who used an

iridium and platinum wire, and enveloped the holder in glass or some other non-conductor. In 1872, Konn employed graphite, and rendered it incandescent in an atmosphere of nitrogen, in which there was no wasting away of the carbon. The same principles have been followed, but with greater promise of success, in the more recent attempts at producing illumination by means of incandescence. The earlier attempts failed, either (1) because of the impossibility of preventing the consumption of the carbon or other material, in consequence of the minute traces of air, which it was impossible to get rid of with the means of exhaustion which were then known; or (2) because of the presence of other gases, such as hydrogen, which exists occluded in platinum and in other substances. It is only quite lately, since our power of obtaining a vacuum has been so greatly extended, and since we have learnt so much about high vacua from the labors of Mr. Crookes, that Mr. Swan and Mr. Lane-Fox have succeeded in obtaining vacua from which all the air and occluded hydrogen are exhausted, so that their carbon filaments and platinum wire connections remain without being destroyed, even when a current of electricity strong enough to make them give out a brilliant incandescent light has been continuously passing through them for months together. Through the kindness of Mr. Swan, and of my friend and former pupil, Mr. Lane-Fox, I am able to show you this evening how well they have succeeded in producing a brilliant, and yet a steady and pleasant incandescent light. This is a triumph which many have sought in vain, and which could not have been attained except by combining together the results of investigations which have been recently carried on in several branches of physics.

I cannot conclude this course of lectures without giving my especial thanks to Mr. H. Trueman Wood, who has given me very valuable assistance, by helping me to bring together a large collection of electrical apparatus, in illustration of the interesting subject which I have had the honor to bring before you.

St. Gothard Tunnel.—The triple concentric vault of the tunnel of the St. Gothard, which was built in the sandy stratum below Andermatt, has been completed and promises to remain as firm as the other parts of the gallery. There is a reasonable hope that engineering skill has triumphed over a difficulty which was at first thought insurmountable.—*Les Mondes.* C.

WEIGHING THE SUN BY A SOAP BUBBLE.

By Pliny Earle Chase, LL.D.

In answer to some inquiries for a fuller explanation of the method of weighing the Sun by a soap bubble,* the following note is submitted to the readers of the Journal of the Franklin Institute.

Naumann ("Handbuch der Chemie," p. 289–90) gives six estimates for the combining energy (Bildungswärme) of a water-molecule, ranging between 67616 calories (Andrews) and 69584 calories (Hess). The mean of the six estimates is 68886 calories, representing a projectile energy, against terrestrial gravitation, of $\frac{68886 \times 1389{\cdot}6}{5280} = 18129{\cdot}55$ miles. As 9 pounds of gas are lifted by 1 pound of combustible, this energy would be sufficient to lift the water vapor $\frac{1}{9}$ of $18129{\cdot}55 = 2014{\cdot}394$ miles.

At the moment of explosion the equilibrium which usually exists between the gravitation of the particles towards the sun, towards the earth and towards each other, is suddenly and violently disturbed. During the restoration of equilibrium there are simultaneous tendencies to the production of orbital velocities about the sun, about the earth and about centres of oscillation.

The explosive force proceeds radially in all directions from the centre, so that the particles are subjected to cones of force, introducing oscillations which may be represented by a synchronous conical pendulum of $\frac{1}{4}$ the height, or 503·599 miles.

In seeking equilibrium the particles tend toward their own centre of gravity at $\frac{1}{2}$ this height, and also, on account of resistance at the earth's surface, towards the centre of linear oscillation, at $\frac{2}{3}$ of the height. Earth's action on the centre of gravity of the mass produces a secondary centre of oscillation, in which the primary centre of oscillation acts as a point of suspension, and the centre of gravity as a pendulum extremity of wave propagation. This secondary centre of oscillation is at $[\frac{1}{2} + \frac{1}{3}$ of $(\frac{2}{3} - \frac{1}{2}) =] \frac{5}{9}$ of $503{\cdot}599 = 299{\cdot}777$ miles, which represents the mean *vis viva* of oscillatory projection, relatively

* See article "Radio-dynamics," in this Journal, July, 1881.

to earth, the *vis viva* relatively to sun being represented by earth's semi-axis major.

Let $r = 3962 \cdot 8$ miles = earth's semi-diameter; nr = earth's semi-axis major; $t_0 = 1$ year = 31558149 seconds; $t_1 = 2\pi \sqrt{\frac{r}{g}} = 5073 \cdot 6$ seconds = time of satellite revolution at earth's surface; m_0 = sun's mass; m_1 = earth's mass; $h = 279 \cdot 777$ miles. Then Kepler's third law and Herschel's principle of forced vibrations give

$$\left(\frac{nr}{r}\right)^3 : \left(\frac{t_0}{t_1}\right)^2 : : m_0 : m_1 : : nr : h$$

Solving the proportion, we find

$$m_0 = 331{,}574\ m_1$$
$$nr = 92{,}767{,}000 \text{ miles.}$$

Five years after I published my deduction of the above ratio between the *vis viva* of wave propagation and the *vis viva* of the oscillating particles, $\frac{5}{9}$, Maxwell published the same ratio in the *Philosophical Magazine* for June, 1877, p. 453, but without stating how he had found it. I have enquired for his method in quarters where I supposed it might be obtained, but I cannot find that he left any record of it.

Struve's constant of aberration gives 497·827 seconds for the time in which light would traverse earth's semi-axis major. Dividing the above value of nr by 497·827 we get 186,344 miles for the velocity of light.

Griscom's Electromotor.—A French writer compares this motor to one of those which have been devised by Deprez, and calls attention to three effects of induction: 1. The inverse effect which results from the magnetization of the iron; 2. That which results from the successive inversions of the polarization of the central nucleus, which Du Moncel has called currents of polar introversion; 3. That which results from the passage of the magnet before the coils, which is dynamic and is of the same character as the one which is developed in the Siemens machine. The last two currents are direct and prolonged during the whole continuance of the motion; the first is instantaneous. When the electro-magnet begins to move the induced inverse current is injurious, but as it does not last the demagnetization is not sufficient to change the attractive impulsion.—*La Lumière Electrique.* C.

ON THE NEW METAL ACTINIUM.

By Dr. T. L. Phipson, F.C.S., etc.

Since the publication of my two notes in the *Chemical News*, vol. xliii, p. 283 and vol. xliv, p. 73, I have made a great number of experiments with the view of isolating the new substance to which the white zinc pigment owes its remarkable property of darkening in the sunlight, returning to its white state in the dark, and not being affected in this manner under a sheet of glass.

These experiments have at last proved successful, and a very short note to that effect was communicated, about a fortnight ago, to the Acadèmie des Sciences and another to the British Association on Monday, September 5th. I will now describe the process by which I have isolated the oxide and the sulphide of the new metal in a state of tolerable purity. Perhaps this process may be improved hereafter, but it is not very complicated, though it has required an enormous number of experiments to arrive at it. First, one word as to the manner in which the pigment found in commerce is prepared. Ordinary zinc scrap is dissolved in sulphuric acid and a considerable excess of zinc is left in the solution in order to keep out iron, lead, arsenic and other metals. The liquid is drawn off and then precipitated by a solution of sulphide of barium; the precipitate is dried, calcined, raked whilst hot into cold water, dried again, ground, etc. It then consists of sulphide of zinc, oxide of zinc and sulphate of baryta, with *minute* quantities of iron, lead, arsenic, manganese, etc.

The manner in which I have obtained the oxide and sulphide of actinium from this pigment is as follows, and the process will doubtless serve for the treatment of other substances in which the presence of the new metal may be detected:

About 15 grammes of the finely pulverized pigment are left for 24 hours in dilute acetic acid (strongest acetic acid and water equal parts), and the mixture well stirred or shaken occasionally. This takes out most of the iron, manganese, magnesia, lime and *oxide* of zinc. The residue, after being washed, is treated exactly in the same manner with dilute hydrochloric acid (acid 8 parts, water 92 parts), with the

object of completing the action of the acetic acid. The residue, well washed, is then heated with strong hydrochloric acid, to which a little nitric acid is added from time to time. The solution of the chlorides thus obtained is filtered to separate free sulphur and the insoluble sulphate of baryta, any remaining sulphur in suspension after filtration being oxidized by a few crystals of chlorate of potash. To this solution of chlorides, somewhat diluted, a considerable excess of caustic soda is added and the solution heated. The zinc oxide goes into solution and the white oxide of actinium remains; the latter is received upon a filter, washed, dissolved in hydrochloric acid and the solution again treated with excess of caustic soda. (These operations may be repeated two or three times, in order to eliminate the zinc oxide as completely as possible.) Finally, the oxide of actinium, still impure, is washed on a filter and dissolved in a considerable excess of hydrochloric acid. The solution is neutralized by ammonia, and then the latter is added in excess. All but a little iron oxide remains dissolved (if not, dissolve again in HCl and add ammonia in excess, which, this time, will only precipitate the iron). The iron oxide is separated by the filter, and to the filtrate sulphide of ammonium is added, which throws down the sulphide of actinium as a bulky pale canary-yellow precipitate, the color of which is best seen when it is received on a filter.

Oxide of Actinium.—The hydrate as precipitated by soda or ammonia forms a bulky white precipitate, more gelatinous than oxide of zinc; unlike the latter, it is only very slightly soluble in caustic soda, even when the liquid is heated; it is not precipitated by ammonia from solutions containing ammoniacal salts. It is a permanent white, with a slight tinge of salmon color when seen in bulk, and it does not change color when exposed to the air, as oxide of manganese does, neither does it appear to be affected by the direct rays of the sun. It is readily soluble in acids. The anhydrous oxide is not volatile nor decomposed by heat. It has a pale fawn-colored tint.

Sulphide of Actinium.—The hydrate as precipitated from its neutral or alkaline solutions by sulphide of ammonium is a bulky pale canary-yellow precipitate, insoluble in excess of sulphide of ammonium, scarcely at all soluble in acetic acid, readily soluble in mineral acids even when they are diluted. When exposed to the direct rays of the sun it darkens and becomes quite black in about twenty min-

utes, except in those places where it is protected by a piece of ordinary window glass.

The quantity of actinium sulphide obtained from the white pigment amounts to no less than about 4 per cent. This yield is enormous. The presence of this new element in zinc will account, probably, for the discrepancies noticed in the *equivalent* of this metal as determined by various observers. The new element differs very essentially from manganese, zinc and cadmium, but has, perhaps, some points of similarity with lanthanum. It exists, evidently, in considerable quantities in at least some kinds of commercial zinc. As soon as I shall have written the next number of my *Journal of Medicine*, I intend to pursue these investigations.—*Chemical News*.

Athermanous Photometer.—The industrial progress of electric light increases the desirability of a good photometer, which is easily transportable and does not require delicate manipulations. Unfortunately, all the instruments which have hitherto been invented have the grave defect of being influenced both by light and heat. Raimond Coulon proposes an apparatus which is acted upon by light alone. It is based upon the following principles: 1. In a Crookes' radiometer, if a difference of temperature is produced upon any point of the surface of the glass envelope the wheel ceases to turn, under the influence of light, as long as this difference continues. There is a fixed relation between the value of the angle which one of the palettes makes with the heated or cooled point, the intensity of the luminous ray, and the calorific difference between the disturbed point and the rest of the envelope. 2. The luminous conditions remaining constant, every radiometer of which the temperature is raised turns in such a manner that the bright side of the mica pallette seems to be attracted by the envelope. Every radiometer of which the temperature is lowered turns in an opposite direction; every radiometer of which the temperature is constant remains immovable, so long as it is in obscurity. 4. Every radiometer of a constant temperature turns under the influence of light alone. The inventor soon found that it was necessary to maintain the instrument at a temperature superior to that which is produced by the radiant heat of the luminous sources which are to be measured. Practically, the temperature of boiling water is convenient and sufficient.—*La Lumière Electrique.* C.

Comets of 1811 and 1881.—Dr. Eugene Robert calls attention to some striking resemblances between the agricultural results of these two comet years. In each of them there was a great drought, lasting through the whole summer, and accompanied, in some countries, with violent storms; the harvest was bad in many places, the crop of cereals especially being short; but the grape crop was good, the fruit being well developed and rich in materials for vinous fermentation.—*Les Mondes.* C.

Ramie.—M. Favier, the inventor of a machine for stripping the bark from ramie, has exhibited his process at Avignon, in the presence of a numerous gathering of industrial and agricultural notables. The experiment was entirely satisfactory; the machine instantly broke the rigid stalks, rejected the woody portion, and transformed the bark into straight, fine and silky filaments, thoroughly prepared for spinning and dyeing. This machine seems likely to add great importance to the cultivation of ramie in the southern and central districts, and, perhaps, even in the northern districts of France.—*Les Mondes.* C.

Radiophony by Lamp Black.—Mercadier finds that lamp black is not only the best thermophonic agent, but that it is also susceptible, like selenium, of becoming an electric photophone. He covers one of the faces of his double spiral receivers with the smoke of oil or camphor, taking care to avoid carbonizing the parchment paper which insulates the metallic spirals. These receivers having two faces, one may be selenized and the other smoked, so that experiments may be made with the same current on either face. Receivers with spirals of copper, brass, iron or platinum, when thus smoked, work well; aluminium receivers, which cannot be selenized, are easily smoked, and then operate readily. With such receivers sounds are easily heard which are produced by the radiations of the sun, of the electric or oxyhydrogen light, or even of a gas jet. Some of the inventor's experiments seem to show that the origin of the sounds is not thermal, in the ordinary sense, but the phenomenon is simply photophonic or actinophonic. The resistance of a lamp black receiver diminishes when the temperature increases. The variation is well represented by a straight line. It is very small, the mean coefficient of variation per degree being less than one-fourth of one per cent.—*Compt. Rend.* C.

Origin of Cometary Light and Heat.—Emile Delaurier advances the hypothesis that the great rapidity of cometary motion causes the æthereal matter to vibrate and transforms it into light and heat. He expands his hypothesis so as to account for all the variations of form which the great comets undergo, for the apparent solar repulsion and for the curvature of the tail.—*Les Mondes.* C.

Application of Electricity to the Study of Rapid Phenomena.—Marcel Deprez has contrived a register which gives excellent results in making rapid measurements with the intervention of a single Bunsen cell. He has already succeeded in reducing the time of a double signal to about $\frac{1}{1600}$ of a second. It is not necessary to have the current closed longer than $\frac{1}{40000}$ of a second in order to give the signals.—*Lumière Electrique.* C.

Comet Spectrum.—The French observers find many striking resemblances between the spectra of Gould's and Coggia's comets. The nucleus gives a continuous spectrum, without bands or lines. The nebulosity near the nucleus shows three bands, one very bright, the others faint. The band spectrum is so much like that of alcohol that Thollon considers them identical. The violet alcohol band is not seen, but its absence seems to be owing to atmospheric absorption. There is little doubt of the presence of carbon in some of its compounds.—*Comptes Rendus.* C.

Explosion of Bubbles.—Plateau has published some new experiments upon thin liquid films. One of the most interesting is one which furnishes conclusive evidence of the contraction which the bubble undergoes during its rapid destruction. He filled with tobacco smoke a bubble of glyceric liquid of about 11 centimetres (4·3 in.) in diameter and laid it upon a ring of 4 centimetres; waiting until the blue color of the summit showed that it was upon the point of bursting he pierced the summit with a metallic wire. The mass of smoke was thrown vertically for about a decimetre above the bubble and then spread horizontally, like an umbrella, and continued to mount more slowly while diffusing itself into the air. The experiment was repeated several times with the same result.—*Acad. Roy. de Belge.* C.

New Method of Telegraphing.—An officer of the French army has invented a telegraph with which some interesting experiments have been recently made at the Trocadéro. It consists simply in reading large letters of silvered zinc, fixed upon a blackened surface. He claims that with an ordinary telescope he can read such despatches, under favorable circumstances, at a distance of 80 kilometres (49·7 miles).—*Les Mondes.* C.

Synthesis by the Electrolysis of Organic Acids.—Bartoli and Paparogli find that in the electrolysis of dilute acids the graphite or carbon electrodes waste away and partly dissolve; with potash the liquid becomes colored; barytes gives $BaO.CO_2$. With a hundred Bunsen elements carbon electrodes become gradually conical towards the lower extremity in distilled water, but they wear away regularly in dilute sulphuric and oxalic acids and in solutions of potash or soda. Mellic and hydromellic acids are found in the solutions. Other solutions give similar effects with the carbons.—*Les Mondes.* C.

Spectroscopic Grouping.—C. L. Ciamician gives a spectrum of the second order for boron, composed of lines in five groups. He groups the elements in the order

B, Al, C, Si N, P, As, Sb O, S, Se, Fe
Fl, Cl, Br. I, Na, K, Rb, Cs Mg, Ca, St, Ba

He thinks that in each group the elements are formed of a fundamental material combined with numerous atoms of oxygen. The atomic weights, which are calculated according to his rules, differ notably from those which are commonly given.—*Ber. Wien. Akad.* C.

Cutting Glass and Porcelain.—In endeavoring to drill glass and porcelain, or to cut them in accordance with marked lines, there are many difficulties which often lead to failure. Messrs. Richter & Co., of Chemnitz, cover disks of soft metal of 15 to 25 millimetres (5·5 to 9·8 in.) diameter with diamond dust, and mount them upon an arbor moving with great velocity, so as to cut glass or porcelain, in a few seconds, according to any given design. By means of cylinders, constructed on the same principle, round holes can be drilled with great rapidity. The wear of the instruments is imperceptible.—*Les Mondes.* C.

Action at a Distance.—An important contribution to photo-dynamics and general radio-dynamics has been made by George Helm, of Dresden. He investigates the movements of molecules in the æther, the exchange of energy between molecules and the external æther. gravitation, magnetics, electrical and dielectic currents and conduction, and the di-electric condition. His results corroborate many of the formulas of Faraday, Maxwell and Chase.—*Wied. Ann.* C.

Height of the Atmosphere.—Dr. A. Kerber has estimated the height of the atmosphere from the phenomena of refraction. By two different methods he obtains heights of 189 and 192·6 kilometres (117·4 and 119·7 miles).—*Wied. Annal.*

[Some of the observations upon meteors and auroras have led to the conclusion that the atmosphere reaches a height of more than 500 miles. Laplace's limit of synchronous rotation would allow a possible height of more than 26,000 miles. The theory of Fresnel and Grove, that the luminiferous æther is only a very tenuous atmosphere, would make the portion which belongs to the earth of the same height as Laplace's limit. C.]

Effects of Lightning upon Trees near Telegraph Wires.—Arago has examined the causes of the dangers to which persons may be exposed in the neighborhood of telegraph wires during a thunder storm. Montigny has lately investigated the danger to trees in similar circumstances. In the neighborhood of Dinant there are nearly 500 poplars on the north side of the road, with a telegraphic wire passing very near them. Eighty-one of these trees, or a sixth part of the whole number, have been struck by lightning upon the south side of the trunk almost always at the point nearest the wire. The trees of the other row, which is at some distance from the wire, are very rarely struck by the electric fluid. The wounds made by the lightning are of three kinds: 1. The bark is torn and shivered upon the side towards the wire over a narrow portion of the trunk; 2. The thunder bolt traces upon the tree a furrow more or less broad which begins at the height of the wire and descends the trunk to the ground, most often in a straight line, but sometimes in a spiral; 3. The wounds have a peculiar oval form, and the edges of the bark are colored a clear brown.—*Acad. Roy. de Belge.* C.

Telegraphy in Paris.—The total number of city telegrams in Paris, in 1880, was 969,177, which yielded a total income of 579,-857.17 francs ($115,971.49). The reduction of the tariff has been found, by various experiments, not only immediately to increase the circulation of telegrams, but also to enlarge the dividends. The number of despatches has nearly tripled within the last four years.—*Bull. de la Soc. d'Encour.* C.

Living and Dead Protoplasm.—O. Loew finds from his investigations that living protoplasm possesses the property, in a high degree, of reducing metals from their solutions, but that this reducing power is lost by death. Hence it may be concluded that the mysterious phenomena of life are connected with peculiar groupings of atoms, possessed of special motions which are analogous to those of the aldehyde group.—*Pflüger's Archiv.* C.

Destruction of Rocks under Water.—Major Lauer, an Austrian engineer, has made some experiments at Krems on the Danube which have excited great attention. He placed a cylinder loaded with dynamite upon the surface of the rock, and exploded it by an electric current. However small the quantity above the cylinder, the rock was crushed into bits so small that they were easily swept away by the current. The process is said to reduce the expense of removing submerged rocks 40 per cent.—*Les Mondes.* C.

Rotation of the Plane of Polarization.—Leo Grunmach reports an investigation which was based upon the supposed identity of the radiations of heat and light, and which leads him to the following conclusions: 1. In solid as well as in fluid diathermanous bodies there is an electro-magnetic rotation of the polarization plane of radiant heat in the direction in which the current flows through the spirals. 2. The magnitude of this rotation is very different for different substances, and appears to be nearly proportional to the index of refraction. 3. In the direct influence of a galvanic current upon the diathermanous bodies the amount of rotation is proportional to the intensity of the current. 4. When a diathermanous body is placed between the poles of an electro-magnet the rotation is proportional to the magnetic force. 5. The amount of rotation increases with the length of the stratum through which the rays pass, but the ratio of increase is not well established.—*Wied. Annal.* C.

Underground Life in England.—The proposed tunnel under the English Channel has led to some statistical inquiries which have shown that the number of persons in Great Britain who are engaged in underground employment is 378,151. The length of the galleries in which their labors are carried on is not less than 58,744 miles. The greatest depth of the channel is 180 feet and the lowest part of the tunnel will not be over 200 feet below the surface. The greatest depth of the coal mines is about 2800 feet and the least is about 300 feet. The channel tunnel will only form about one-thirtieth of one per cent. of the total subterranean excavations.—*Les Mondes.* C.

Conclusions of the Electrical Congress.—The electrical congress at Paris has adopted the following conclusions: 1. The fundamental unit is the centimetre, gramme, second (C. G. S.); 2. The *Ohm* and *Volt* will retain their present values, 10^9 for the ohm and 10^8 for the volt; 3. The unit of resistance (*Ohm*) will be represented by a column of mercury having a section of a square millimetre and the temperature of 0°C.; 4. An international commission will determine by new experiments the length of the above column of mercury which will represent the value of the Ohm; 5. The current produced by a Volt in an Ohm is to be called *Ampère;* 6. A Coulomb is the quantity of electricity which would enable an Ampère to give a Coulomb per second; 7. A *Farad* is the capacity in which a Coulomb gives a Volt.—*La Lumière Electrique.* C.

Book Notices.

A Study of Various Sources of Sugar; Sugar-cane, Sorghums, Sugar-beet, Maple, Watermelons, etc. By Lewis S. Ware, Member of Am. Chem. Soc., etc. Philadelphia. 8vo. H. C. Baird & Co. 1881.

This thick brochure, containing some extensive tables, shows much knowledge of the subject treated, and acute investigation; and it well deserves the attention of all who desire to ascertain the fullest material progress of the sugar interests of the United States. The main intention of Mr. Ware is to advocate the extensive manufacture of

beet-root sugar, which has—as to this staple—rendered France independent of all countries, and which is widely and successfully introduced throughout Europe. This book shows, *inter alia*, that in a field of operations of equal extent the beet would have advantages over cane sugar. The cost of cultivation of 100 acres of sugar-cane and of beet-roots would average nearly the same (about $50 each per acre); but while the net profit on the *manufacture* of cane sugar would be at the rate of $20 per acre, the net profit on beet sugar would be $46 per acre. One-third only of such a large difference ought to prove a wonderful stimulus to the introduction of beet sugar.

The value—including custom dues—of the *foreign* sugar consumed in the United States during the years 1877, '78 and '79 is shown to average $109,000,000 annually, and for 1880 the amount was $118,749,573. All this vast yearly expenditure might go to our own agriculturists, refiners, and their employés. As sugar from beets has been proved to be equal to that made from cane, the relative economy of manufacture must largely influence the future introduction of beet culture. To this end, however, the dissemination of proper information is needful, that farmers may cultivate with confidence, and refiners contract freely for the production.

Another intention of this book is to prove how illusory and unreliable were the favorable reports made by the Agricultural Bureau of the United States respecting sorghum cane and its saccharine qualities.

Mr. Ware's book is a timely contribution to the technical literature of this age of many transitions. S. H. N.

Modern Milling. Being the Substance of Two Addresses delivered at the Franklin Institute by Robert Grimshaw, Ph.D. 8vo. Philadelphia: H. C. Baird & Co. 1881.

This work does not pretend to enter deeply into the multitudinous subjects of flour mills, flouring machinery, and the various processes. It gives, however, an excellent idea, in popular form, of the most modern plans for producing flour, and the requisite improved machinery. A thorough treatise on flouring mills and machinery is much needed, none such, we believe, existing in any language. This is not

surprising if it be considered how the flouring processes and machinery have been changing during the past ten years; it would have been almost impossible for any writer to keep pace with the improvements.

We understand that Mr. Grimshaw has now in press a large and complete work on this important industry. If such had not been the case, the present book would have been disappointing; but its contents fully agree with the statements of the title-page, and as an introductory volume it is good. The first portion treats of the form and various envelopes of the wheat berry; the ideal mode of splitting it in gradual reduction; also descriptions, with illustrations, of smutters, separators, heaters, and other grain-preparing machinery. Several modern arrangements of buhrs are shown, their ventilation, etc., followed by an excellent description of the machines for purifying middlings, such as have been in use for some years past. A good feature in the book is a concise summing up of the various changes in processes, machinery, customs, size of buildings, etc., which have occurred during the past ten years.

High *roller* milling is the subject of the second lecture, and in it the various forms of rollers, their grooving and mode of action, is clearly explained. At the close of the volume there are several large plates, showing the interiors of typical modern flouring mills. S. H. N.

Progressive Agriculture. By C. L. Ingersoll. The Relation of Science to Agriculture. By Harvey W. Wiley. 8vo. Indianapolis. 1880.

These two essays upon very important subjects have been reprinted in pamphlet form from the Proceedings of the Indiana State Board of Agriculture. The authors have the chairs of Agriculture and Chemistry in the Purdue University, and the subjects are treated in an able way, Prof. Ingersoll insisting that progressive agriculture demands for further progress a more thorough education of the masses in the underlying principles of the science and their best application, also to abandon the hap-hazard style of farming, for in order to succeed the farmer must systematize his labor, the rotation of crops, etc., to place his productions at the least cost, remembering that in unity of thought, purpose and action lies the road to success.

In Prof. Wiley's contribution he takes up the subject of the devel-

opment of agriculture, as it is only in this way that an idea can be formed of the processes which have been and are most active in determining agricultural progress, confessing that the advance for two thousand years has been evidently small, and hardly any record of it preserved, the great chemist Liebig being the first to promulgate the principles of modern scientific agriculture. A *resumé* is then given of the scientific views held with reference to soils, crops and fertilizers, the influence of humus, etc. Next to the soil, climate and the weather are claimed as the most important factors which enter into the agricultural problem, and a fitting compliment is paid to the work of the signal service for the remarkable accuracy of many of its predictions. The paper closes with short accounts of the part biology plays in the study of the domestic animals and the laws of their variation and improvement; the intimate relations botany and entomology have to agriculture, and last, but not least, chemistry, the science which has always been regarded as bearing the most intimate relation to the culture of the soil, and yet, candor compels him to own, as not being the most important as far as the practical work of the farm is concerned, a knowledge of the soil and its constituents not being able to solve every problem, and much of the disappointment at the results of chemical methods and of the ill will it has received arising from this cause. Chemistry alone will not do everything for the farm, but when its work is properly combined with the work of of other sciences, it first acquires its true value. I. N.

The Preparation and Use of Cements and Glue. By John Phin. 12mo. New York: The Industrial Publication Co. 1881.

In the preparation of this little book, we are assured in the preface, that the utmost care has been taken to secure accuracy. When we consider the number of utterly worthless recipes which have been published with the sanction of names which stand high in the mechanical world, it is no wonder that even the best collections have admitted useless formulæ to their pages, and the compiler adds that he believes most of the formulæ here given will do all that is claimed for them, and wherever he has had any doubt upon this point it has been intimated, the reliability of many of them having been tested by personal experience.

Such information is of great value, and to have it collected in a neat and handy little volume, without searching for it in the innumerable journals, cyclopædias, etc., will be appreciated by the busy workman and amateur mechanic, into whose hands this work is likely to fall. One hundred and seventy-seven formulæ in all are given, alphabetically arranged for ready reference, while the general rules for the use of cements, with which the book opens, seem mest excellent.

I. N.

THE ELEMENTS OF PLANE ANALYTIC GEOMETRY. By George R. Briggs. 12mo. New York: John Wiley. 1881.

Newton so well established his principles of conic sections that there has since remained but little place in that branch for modern authors. New examples and explanations may be given by them, but the methods and the results remain unaltered.

We bslieve that too little attention has hitherto been accorded in school text-books to the *Loci*, which is the most important and useful of all special applications of mathematical principles. Knowing the conditions of a moving body, its law of motion, its path through space or on a plane may be determined by an algebraical formula whose co-ordinates satisfy an equation known as its *Loci*.

This subject has been briefly but intelligently handled in Mr. Briggs' little book, and we are convinced it must be a valuable assistance to students of the Freshman class of Harvard College, as well as to adults who are lovers of mathematics in its elementary branches.

L. S. W.

Franklin Institute.

HALL OF THE INSTITUTE, Nov. 16th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 102 members and 45 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers and announced that at the last meeting of the Board 26 persons were

elected members of the Institute; also that, in accordance with the recommendation of the Committee on Science and the Arts, they had awarded the Elliot-Cresson Gold Medal to William Wooduut Griscom for his invention of the Electric Motor.

Prof. Robert E. Rogers, chairman of the Committee on the "Dangers Incident to Electric Lighting," said that he regretted to be obliged to say that, while his committee had held several meetings, they had not been able to prepare an absolute and complete report to be presented at this meeting. In view, however, of the apprehensions and anxiety of the community, which looks to the Franklin Institute for guidance on this subject, the committee desired permission to give its report to the press and public as soon as it should be completed. "It is due to the Institute," he said, "that the subject should not be trifled with, for it is one of great gravity, and the report of the committee should be well considered. The committee would pledge itself to present its report in such a way as to furnish full information to the public and yet not commit the Institute to its views until action could be taken on it."

Mr. Orr said that the chairman of the committee evidently took a true view of the importance of the subject committed to its consideration, but he hoped that if the report was to be given to the public before being acted upon by the Institute, that fact would be distinctly stated.

Prof. Rogers said that he proposed to make the committee alone responsible for the views expressed, and not the Institute, until the latter had been given an opportunity to pass upon it.

On motion, the committee was given leave to present its report through the press, as suggested by Prof. Rogers.

The report of the Committee on Fire Escapes (which is printed in full in this number of the JOURNAL) was then read by the Secretary.

Mr. Grimshaw said that he considered the report most excellent and timely, but he wished to enter his individual protest against one or two of the conclusions of the committee. He protested against the endorsement of bridges leading to tower stairways, believing that the bridges were liable to be blocked up by people attempting to escape by such long tubes. He also wished to protest against the recommendation to enclose elevator shafts in brick walls, thus making them flues to carry the products of combustion over the building.

He thought that elevator shafts should not be enclosed. He also called attention to the fact that the words "incombustible" and "fire-proof" were not interchangeable, and said that a stairway built of incombustible material as recommended in the report might, nevertheless, be dangerous in case of fire, being crumbled or broken by heat.

Prof. Rogers said that he considered the report admirable; as nearly exhaustive as it could be made, and free from the objection of committing the Institute to any special form of fire escape. It tells frankly the dangers to be met, describes various ways of meeting them, compares their relative merits entirely on general principles and from all the information deduces certain guiding lines of action in legislation and in the construction of fire escapes. He felt so well satisfied with the work of the committee that he offered the following resolution:

Resolved, That the thanks of the Institute be returned to the committee for their able report, and that the Institute adopt it with the provisions and qualifications contained therein.

This was agreed to by a vote which appeared to be unanimous.

Mr. Hugo Bilgram read a paper on his new Odontograph, illustrated by a model showing how it could be used for laying out the teeth of gear wheels. It is based on the fact that if the shape of the teeth of a rack consists of two congruent branches, corresponding points of which meet tangentially in the pitch line, any two gear wheels correctly gearing into this rack will also correctly gear with one another. Briefly described, a template of a tooth, carefully filed so that each side consists of two equal curves meeting exactly in the pitch line, is attached to a bar representing the rack, one edge of which (the pitch line) is carefully straightened. The template is so attached to the bar that a small space is left between them the depth of the tooth. A circular plate (or portion thereof) of a diameter equal to the pitch diameter of the wheel, has fastened to it a piece of sheet metal or card-board on which the tooth is to be marked and which must, therefore, be as large in diameter as the gear wheel. When the wheel is rolled upon the rack, pitch line on pitch line, the sheet metal or card-board of the wheel passes between the template of the rack and the bar to which it is fastened, and the shape of the tooth can be described upon the second template in a number of different positions. The form thus delineated being then cut and filed the second template

is completed. A band of very thin steel, one end of which is fastened to the rack and the other to the wheel plate, may serve as a means to prevent slipping. It is advisable to have the second template extend to the centre of the plate, that its centre may be marked to insure radial position of the teeth. Mr. Bilgram also explained at length how this principle could be applied to machines for cutting gear wheels.

The President of the Institute described his Improved Dynamometer, a large model of which was on exhibition and photographic diagrams of three forms of it projected upon the screen. It aims, by a simple and novel device, to measure the power transmitted without absorbing it. A description of the apparatus is published in the JOURNAL for November, with illustrations.

The Secretary then presented his report. Among the novelties shown was Chamberlain's Safety Attachment for Elevators, designed, in case of the breaking of an elevator rope, to instantly stop the car from falling before it has acquired any momentum. The rope is attached to a yielding draw bar or cross bar, connected by links with a rock shaft under the car. Locking cams act against vertical chains of rods, which are firmly attached at their upper and lower ends to rigid parts of the elevator well and pass through grooved recesses in the elevator car. When in use the cams are turned so as to allow the car to move freely up or down; but the moment the rope breaks, thus removing strain on the cross bar, springs on the rock shaft are freed and operating upon the shaft turn the cams into a position where they are at once engaged in the locking recesses. The inventor claims that this action is instantaneous, and that there is consequently no opportunity for the car to acquire momentum.

A balanced valve for steam engines, the invention of Mr. William L. Dewart, Jr., was described, which has been in use at the Philadelphia and Reading Railroad shops on Hamilton street for the past year. The object is to lessen pressure of the valve upon the valve seat, thus diminishing friction and effecting a saving in the cost of a given amount of power. In this valve there is a superimposed balance plate connected with the body of the valve by a ball and socket joint, the plate and valve being so constructed that they compensate for any inequality in the valve seat. The inventor claims that with this balanced valve there can be no choking nor back pressure, nor

can there be a "cushion" formed between the valve and the sides of the chest.

Two of Holman's new Projecting Lanterns, made by Zentmayer for Mr. Henry Bower and Mr. George H. Perkins, were exhibited. They are intended to be used in the lecture hall, school room and the home, to show more than one person at a time the revelations of the microscope, and also to serve as first-class microscopes for scientific investigation, and as lanterns for the exhibition of ordinary slides. Mr. Holman stated that with one of these instruments the little animal *amoeba* had been for the first time shown to more than one person at a time, this being done in a large lecture hall in illustration of one of his lectures.

A number of fire escapes were shown, among them being a truck ladder invented by Henry C. Bender, which can be taken to a burning building by the firemen and raised to any story against or independently of the wall. The ladder is made in links so as to roll up on a cylinder. The turning of a crank causes it to extend and rise from the truck as a rigid ladder. The truck itself can be shifted laterally while the ladder is extended.

One of Crookes' Ruby Tubes, imported by Queen & Co., was exhibited by the Secretary; also a model of Grier's Street Railway, in which the cars are propelled by an endless wire rope (operated from a central station), to which the cars are attached by a clutch passing through a narrow opening in the street.

Mr. Cooper said he would like to bring before the Institute the question of holding an exhibition of small machines this winter, especially of such as are of general interest, including electric lights and other electrical apparatus. He had noticed that Industrial Hall on Broad street had been recently enlarged, and he thought that the Institute could hold a creditable and useful exhibition there of the kind suggested.

The President inquired whether Mr. Cooper had any motion to make and, when told he had not, said that if Mr. Cooper would present his suggestion to the Committee on Exhibitions he had no doubt it would receive attention.

On motion, the Institute adjourned.

ISAAC NORRIS, M.D., *Secretary*.